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Notices of the Aëronautical Society of Great Britain.

May Elections.

Member: Jan Schiere.
Associate Member: Lieutenant-Colonel Stanhope Pedley.
Foreign Member: John Gadsby.

June Elections.

Members: Colonel the Rt. Hon. J. E. B. Seely and John H. Patterson.

Next Fellows and Associate Fellows Election.

Application forms for the next election of Fellows and Associate Fellows, which will take place shortly, can now be obtained from the Secretary, and it should be noted that it is not necessary that the applicants should be Members of the Society.

Full details will be announced in the Technical Press.

Joint Committee with the Royal Meteorological Society.

The following gentlemen have been appointed to serve on the above Committee:—Dr. T. E. Stanton, F.R.S., Lieutenant-Colonel F. H. Sykes, Alec Ogilvie, and another, representing this Society; C. J. P. Cave, M.A., Dr. C. Chree, J. S. Dines, and G. K. Lempfert, representing the Royal Meteorological Society; Colonel H. E. Rawson, C.B., is the Chairman of the Committee, the work of which is now being inaugurated.

Award of the Society's Gold Medal.

As will be seen by the separate announcement, the Gold Medal of the Society has been awarded to Professor G. H. Bryan, Sc.D., F.R.S., for the great services he has rendered to Aeronautics by his development of the theory of the Stability of Aeroplanes.

Wilbur Wright Memorial Banquet and Lecture.

A Report of the second Wilbur Wright Memorial Lecture appears in this issue. The Banquet was held at the Royal Automobile Club, at eight o'clock on the evening preceding the lecture, and was most successful. The Society is much indebted to the Sub-Committee (Major-General R. M. Ruck, C.B., Mr. Griffith Brewer, Mr. Mervyn O'Gorman, C.B., and Lieutenant-Colonel F. H. Sykes), who undertook the arrangements.

The list of official guests is given below:—

A. J. Atkinson ("The Standard"), Leonard Bairstow, A.R.C.Sc., H. T. Baker, M.P. (Financial Secretary, War Office), Sir William Beardmore, Colonel Bingham, Sir R. H. Brade, K.C.B. (Permanent Under-Secretary, War Office), Thomas Baldwin, A. Graham Clark (Technical Editor "Flight"), B. G. Cooper (Secretary, Aëronautical Society), J. S. Critchley (President, Institution of Automobile Engineers), Sir Hay Frederick Donaldson, K.C.B. (President, Institution of Mechanical Engineers), Wm. Duddell, F.R.S. (President, Institution of Electrical Engineers), Sir Archibald Geikie, F.R.S., C. Gordon Bell, Brigadier-General D. Henderson, C.B. (Director-General of Military Aeronautics), J. E. Hutton (Chairman, Aero Committee, Society of Motor Manufacturers and Traders), "Morning Post," F. Grant Ogilvie, C.B. (Director, Science Museum), Rear-Admiral Sir Charles Ottley, K.C.M.G., J. H. Patterson, Harold E. Perrin (Secretary, Royal Aero Club), Press Association, Colonel the Rt. Hon. J. E. B. Seely, D.S.O., M.P., Hon. Arthur Stanley, M.V.O., M.P. (Chairman, Royal Automobile Club), Brigadier-General Guthrie Smith, H. J. Tennant, M.P. (Parliamentary Under-Secretary of State for War), Charles C. Turner, Major-General S. B. von Donop, C.B. (Master-General of the Ordnance), H. Walter ("The Times"), B. Woodward (Hon. Solicitor, Aëronautical Society).

Wind Tunnel Tests of Model Aeroplanes to ascertain Best Lift/Drag Ratio.

The following are the rules of the competition to be held in conjunction with the Kite and Model Aeroplane Association this year.

Tests under the auspices of the Aëronautical Society of Great Britain for prizes to the value of £5, given by the Society.

1. The span of the model to be 24 ins.

2. No model will be accepted that has not flown at least 150 yards in the open, allowing for wind velocity under observed conditions.

3. The models entered will be retained by the Kite and Model Aeroplane Association after the flying test until they have been tested in the Laboratory.

4. Models must have propellers fitted with bearings of such design that they will revolve freely when the rubber is detached.

5. The rubber will be kept on the model during test in approximately its flying position, although detached from the propeller.

6. A special fitting must be affixed to the model so that an attachment may be made, from the under-side of the model, to the balance arm of the wind tunnel. The design of this fitting will be published later.

7. No responsibility for damage to models either in test or transit is taken by the Kite and Model Aeroplane Association or the Aëronautical Society.

8. The first prize (£3) will be awarded to the model giving the best lift/drag ratio at a wind speed of 18 miles an hour in the tunnel, and the second prize (£1 10s.), and third (10s.), prizes for the next best results.

The Lanchester Trophy for Model Gliders.

Mr. F. W. Lanchester has asked the Society to undertake the awarding of an annual trophy given by him for the purpose of encouraging research into the best shape of aerofoils. Full details will be announced later.

Library.

The Council desire to thank Mr. Patrick Y. Alexander for thirty copies of "Chronology of Aviation," by Hudson Maxim and William J. Hammer, and also the Royal Institution of Great Britain for the following:—

"Photography of the Paths of Particles Ejected from Atoms." C. T. R. Wilson, M.A., F.R.S.
"Gyrostats and Gyrostatic Action." Prof. A. Gray, M.A.
"The Winds in the Free Air." Charles J. P. Cave, M.A., J.P.
"Fluid Motions." The Rt. Hon. Lord Rayleigh, F.R.S.
"The Gyrostatic Compass and Practical Applications of Gyrostats." G. K. B. Elphinstone, M.I.E.E.
"Travelling at High Speeds on the Surface of the Earth and Above it." Prof. H. S. Hele-Shaw, LL.D., M.I.C.E.
"Icebergs and their Location in Navigation." Prof. H. T. Barnes, D.Sc., F.R.S.
"Recent Advances in Scientific Steel Metallurgy." Prof. J. O. Arnold, D.Met., F.R.S.
"The Pressure of a Blow." Prof. B. Hopkinson, M.A., F.R.S.
"Recent Advances in Turbines." Hon. Sir C. A. Parsons, K.C.B.

OBITUARY.

WILFRID DE FONVIELLE.

Wilfrid de Fonvielle died on the 29th of April, at Paris, aged 88 years. He was one of the fast dwindling band of early aeronautical pioneers, who have had the felicity of seeing their aspirations of so many years take concrete form and commence to become part of the every day life of the world.

The part which he played in the long campaign of argument and experiment that was to vindicate the claims of those who claimed flight to be possible was characteristically energetic, and his output of articles and books was tremendous. A life so full of incident cannot be adequately summed up in a few words. Deported at the time of the Coup d'Etat of 1852 to Algeria, for his Republican sympathies, he afterwards went to London and studied at the Royal Institution. Afterwards returning to France he occupied himself with electricity for some years. In the war of 1870-71 his aeronautical studies stood France in good stead. In company with Mons. Tissandier he offered at the outset to accompany the army of the Rhine with captive balloons. It is said that this offer was not even acknowledged by the Powers that were, but it was not long before the beleaguerment of Paris by the victorious Germans made the balloon the only means of communication with the outside world. A balloon service was formed by Mons. de Fonvielle and others. He escaped from Paris in a balloon at 10 a.m. on the 24th November, 1870, and 4¼ hours later landed at Louvain in Belgium. The balloon service which he helped to found was most successful, as is well known, and it is small wonder that with such an example always in his mind Mons. de Fonvielle remained an enthusiastic partisan of the "lighter-than-air" to the day of his death.

He was one of the moving spirits of La Société Francaise de Navigation Aérienne, of which he was an ex-President, and a Membre d'honneur of the Aéro Club de France. He was also an enthusiastic member of the Société Astronomique de France.

In 1901 Mons. de Fonvielle came to London at the invitation of the Aeronautical Society to deliver a lecture on "Aeronautics in France," and it is interesting to remember in passing that at that time "L'Entente Cordiale" had not yet come into being; in fact, owing to the Boer War the relations between the two countries were not of the pleasantest. However, science knows no frontiers and the visit was most successful, as may be seen by referring to the report in the AËRONAUTICAL JOURNAL, July, 1901. Mons. de Fonvielle was made an Honorary Member of the Society, thus signalising the friendly relations between the French and English societies that have always existed.

At the Council Meeting of the 10th June, 1914, the following resolution was passed:—

> "That the Council of the Aeronautical Society of Great Britain has learnt with great regret of the death of Mons. Wilfrid de Fonvielle, an Honorary Member of the Society since 1901, and begs to tender to his family and to his colleagues of the Société Francaise de Navigation Aérienne and l'Aéro Club de France, sincerest sympathy in their great loss."

GUSTAV HAMEL.

BORN JUNE 25, 1889. LOST AT SEA, MAY 23, 1914.

By general consent, the loss of Gustav Hamel is the biggest personal loss British aviation has sustained. The Admiralty signalised this in a very uncommon

way, on the abandonment of the search for any trace of Hamel in the Channel, by issuing an announcement, in the course of which they said :—

> He was without question the foremost exponent in these islands of an art whose military consequence is continually increasing. His qualities of daring, skill, resource, and modesty merited the respect of those who pursue the profession of arms.

The King and Queen wrote to the bereaved father a letter, in which their Majesties said they knew Gustav Hamel personally,

> and were struck by the skill, courage, and mastery with which he controlled the aeroplane, no less than by his modest and unassuming bearing.

He was mourned as few have been; and it is but right that, in placing a brief account of his career upon record, the reasons for this should be analysed.

Gustav Hamel never sought the popularity he won. He never resorted to aerodrome affectations, which are so painfully obvious in some fliers. Trying to solve the reason for his success, a mutual friend once said to me: "It is because he is artless. The public are so thoroughly weary of the methods of certain men that they give all their support to the man who does not stoop to them." No doubt this counted; but then there are, fortunately, many aviators who do not resort to the common tricks of the publicity seeker, so that the explanation is inadequate. Hamel was seldom interviewed for the newspapers. His sole appeal to the public, apart from his flying, was his book written in collaboration with myself.

As he appeared to the public, who saw him flying at Hendon and elsewhere, he was unquestionably a more brilliant pilot than any other; but, after all, the actual difference between him and many another was not enormous, and that little difference surely does not account for the very much higher place he had in their regard. Certainly he gained a reputation for punctuality, and he always endeavoured to give rather more than was expected of him; and this counts for a good deal. This is, indeed, admirable, especially in a man who is known to be popular in the "higher circles," as Hamel was. But the public also loved his youthfulness, and were taken by his unaffected manner and his "looks." He looked, and he was, clean. He was just a public school boy. He was liked by men who were supposed to be his rivals; and never was there a brilliant flier who was less the object of professional jealousy.

To pass from the lovable personality of the man to the work he did; I count it, if anything, in his favour that he broke no world's records. He could have flown the fastest death-trap of an aeroplane ever made; but I rather doubt if he could have beaten the duration record, or the altitude record of 20,000 feet or so. The latter, possibly. At any rate, these things never appealed to him: he was not deeply impressed with their importance. Yet was he master of his art, and cared little how hard it blew or rained provided he could get an occasional glimpse of earth. For four years he was ever on the wing, and he must have flown tens of thousands of miles. He could, aerially, read much of England and France like a book.

It were almost idle to speculate on the cause of his disappearance, but I decline to believe that he was guilty of very bad judgment in attempting to cross the Channel even in the bad conditions that prevailed on May 23. That he made a mistake somewhere is undeniable. But he knew the risks well, and had overcome treacherous conditions again and again. His machine was so fast that the crossing should have taken no more than twenty minutes. It had served him well all day, and he had had a foretaste of contrary winds: he had not been lulled into a false sense of security. For these reasons we must acquit him of any greater folly than that inherent in essaying the crossing in any circumstances on a machine that will not float well. He had often told me that it was a risky thing to do;

and yet we find him doing it on May 23. If one of us, his personal friends, had been there with him, he might have been induced to delay the crossing. But confidently, bravely, alone he made his last ascent, gazed around and below at the chequered plains of France dimly seen through the mist, at thick clouds driving up Channel and hiding it; and he estimated the conditions and the risks, and headed towards England.

To sum up Hamel's work in a sentence, it may be said that he advanced the art of flying generally, and in certain quite definite details. It was characteristic of him that, although not the first to "loop the loop," when he did that feat it was on a standard machine of high power, and, in its way, a very definite piece of pioneering. And when he did "loop the loop" it was with a mastery that eclipsed most other fliers. His side overturns and his tail-slides were astonishing. In all these things he was adjudged the equal of Garros. But far more than in feats of this kind did his service to aviation consist. And it is a splendid consolation, now, that he was able, before he passed from our ken, to place a good deal of the experience that he had won in peril from the air upon record for the benefit of other fliers.

It came as a surprise to a great many, even of his friends, that Hamel, who talked so little and never expatiated on the scientific aspects of flying, nevertheless condescended to book-writing. When I proposed the matter to him in May, 1913, he welcomed the idea enthusiastically. So we set to work. He had no experience of book-compilation, but here my training, such as it is, came in, with the advantage that I knew, at any rate, the rudiments of aeronautics and of the practical art of flying. I was able to draw up a fairly workable scheme and obtain and classify a good deal of the tremendous amount of detailed knowledge that Hamel had accumulated, and over which he cogitated and pondered far more than people who only saw him fly could possibly imagine. I could never suggest a point to him that failed to draw some valuable contribution. A great deal of his experience is that of others, of course, but the fact that many very experienced aviators have told me that they find much to learn in the book proves how very necessary was this first really serious work dealing with practical piloting. It was nothing like the book we mapped out, but no book ever is. But its reception by all the reviewers, and especially by those who are competent from an expert point of view, showed Hamel, it is pleasant to know in time, that he had done well. We had an ambitious programme of flying experiments to carry out for a further work; but all this, alas! is now abandoned.

In the preparation of the book Hamel worked hard. Chiefly it was by means of long consultations together, afterwards materialised in manuscript, which he and I read together. On a number of subjects he wrote notes, some detailed, others sketchy, for me to elaborate. I should like you to leave off reading this with a glimpse of Hamel, a man made much of by society, and enjoying that side of life in youthful good spirits, but spending happy, satisfying hours hard at work on the book.

It was Hamel's scheme for crossing the Atlantic that almost shook even my scepticism on this subject. I really think, with the forethought and the elaborate organisation he was putting into it, that he might have been the first to fly across "the herring pond." Contrary to the belief of many who did not know him, he was going to leave very little indeed to chance. It was a sound scheme; but of course it would not have ushered in a weekly aeroplane trans-Atlantic service, any more than will any other scheme that may happen to succeed.

Hamel had yet to do much for flying. Let no man say his work, great as it was, had finished. There were matters toward beside which even that which he did would fade into comparative insignificance.

CHARLES C. TURNER.

AWARD OF THE GOLD MEDAL OF THE AERONAUTICAL SOCIETY OF GREAT BRITAIN TO PROFESSOR G. H. BRYAN, F.R.S.

It is fortunate, when virgin fields have to be explored, that of the pioneers who come forward to show the way over the uncharted ground some not only blaze a trail themselves but make it possible for others to complete, so clear has been the lead that has been given, the mapping of the new territory.

Such a pioneer is Prof. Bryan. The exploration of the stability of aeroplanes has been tackled by many pioneers, but it fell to one to apply to the problem the masterly method expounded in "Stability in Aviation."

The mathematical mill will only give results commensurate with the material fed thereto, and consequently the master mind of mathematics in its practical applications is that which sees clearly what kind of mill to use and what to put into it. This once done the rest may be said to be easy. Certainly it is "hack-work" compared to the first steps. The particular mathematical mill that Prof. Bryan saw was most suitable for experiments on aeroplane stability was due to the labour of several eminent minds, foremost of whom was Routh. It was ready to hand for others to use, but they did not do so. Bryan had the perception to see its suitability, and the courage and pertinacity to experiment, both with models and with equations, until he had fitted the latter to the former and shown what deductions could be drawn.

It was a work of genius in every sense of the word, and it is not surprising that only few saw its worth at first. Hostility to mathematics in general, lack of certain experimental data that Bryan had not the time or opportunity to obtain, these and other causes made it appear to many as if his work was after all only a mathematical tour de force of small practical utility. It is a matter for congratulation, therefore, that the value of Bryan's work was appreciated in the quarters where time and money were not lacking to supply the data required to vindicate its practical use. These data have now been or are being supplied to cover the whole ground of aeroplane stability, and Bryan must perforce stand aside and leave to others the training of the lusty infant he has begotten. As far as showing his fitness for the honour done him by the Aëronautical Society he need do no more—as the Americans say, he has "made good."

It would be interesting to know how many of those who heard the lecture by Bryan and Williams to this Society in 1903 on "The Longitudinal Stability of Aeroplane Gliders" (AËRONAUTICAL JOURNAL, January, 1904, pp. 12-17) realised that there were the germs of the method that was to reduce the elusive and mysterious "stability problem" to systematic order before many years were up, or that at their meeting on the 10th June, 1914, the Council would decide to award the Gold Medal of the Aëronautical Society of Great Britain to Prof. Bryan "for his great services to the science of aeronautics." Certain it is that many would have scouted the possibility.

The actual presentation will take place next Session, on a date to be subsequently announced, when it is hoped that members will turn up in force to honour their colleague. Previous recipients of the Gold Medal, the highest award of British aeronautical science, are Wilbur and Orville Wright, 1909, and Octave Chanute, 1910.

The FOURTH MEETING of the FORTY-NINTH SESSION of the Aëronautical Society of Great Britain was held at the Royal United Service Institution, Whitehall, S.W., on Wednesday, 7th January, 1914, at 8.30 p.m. Major-General R. M. Ruck, C.B., presided. Members of the Royal Meteorological Society were present as the guests of the Society, and a joint discussion took place. Light refreshments were served at the conclusion of the meeting.

WIND GUSTS AND THE STRUCTURE OF AERIAL DISTURBANCES.

BY W. N. SHAW, LL.D., SC.D., F.R.S., FELLOW.

Aeronautics is certainly the science of the future and its professors and preachers have the qualities of their science. When you were good enough to ask me to read a paper, by way of opening a discussion this evening, you told me beforehand the subject which I should select. It is a new departure, characteristic of these new times. If you will look at the practice of education in this or any other country you will find that hitherto subjects have been selected, not by those who are to hear, but by those who are to speak. They are the subjects upon which the teacher has already made up his mind. The principle of selection is that the teacher must be able to display superior knowledge and the process of education is a sort of wave of superior knowledge going down from the universities into the most elementary recesses without much regard for what it looks like from the point of view of those hungry for the knowledge of "how it goes," and still more "why it goes?"

You have provoked this digression by giving me the subject, "Wind Gusts and the Structure of Aerial Disturbances." I really do not know whether I can do justice to it; and I am provoked to a further digression by another characteristic of futurity which might be called pre-maturity. In one of the announcements of this meeting in the papers I notice that what I have to say about gusts and aerial disturbances has already been characterised by some suitable adjective—I forget what it was.

It is a little depressing to be told beforehand, down to the last adjective, what particular kind of boredom one is going to inflict on the audience that might in self-defence have stayed away, but I ought not to complain. I am a forecaster myself, and the forecast of this evening's gloom gives me a fellow-feeling with the weather, if, when it thinks as I do now, that it is doing its best to be fine and bright and pleasant, it comes across a forecast from my office which turns its pleasure into patient endurance by some such adjective as "dull, unsettled."

All this is because I find "wind gusts and the structure of aerial disturbances" a very difficult subject and I hardly know how to get to close quarters with it.

Gusts and Squalls (Figures 1 to 11).

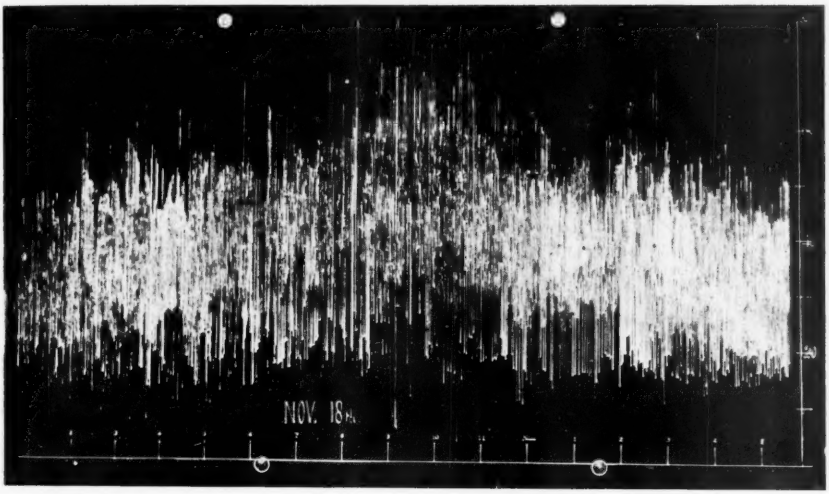

FIGURE 1. The gustiness of ordinary wind represented by a reproduction of the record of the Stokes "Bridled Anemometer," a cup anemometer the spindle of which is constrained by gear which allows it to turn through an angle proportional to the mileage of the wind. The trace was obtained at Holyhead on November 18, 189? The mileage is indicated by the figures on the right of the figure. The graduations at the bottom show the hours. The ordinary range of gustiness shown on the trace is between 15 miles per hour and 50 miles per hour. There are occasional excursions to nearly 80 miles per hour.

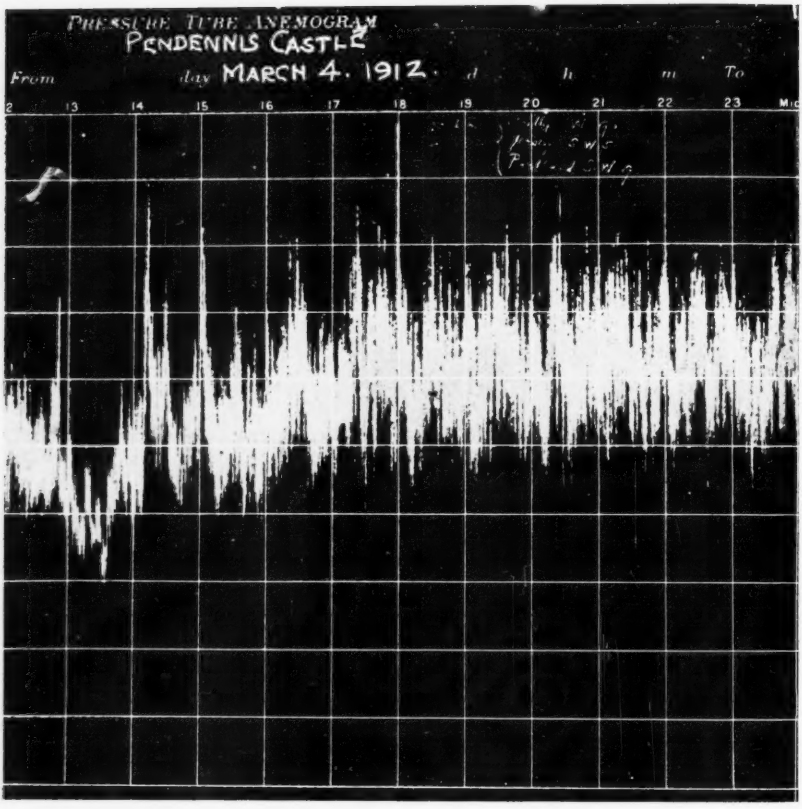

FIGURE 2. Wind gusts and squalls at Pendennis Castle, Falmouth, recorded by the pressure tube anemometer on March 4, 1912. The graduations marked along the top are hours. The horizontal ruling is for intervals of 10 miles per hour. The mean velocity of the wind is about 62 miles per hour, and the range of gusts and lulls from 50 miles per hour to 75 miles per hour. But besides the gusts of very short duration there are occasional squalls lasting about five minutes, and reaching in one case 87 miles per hour, and fluctuations of the mean wind velocity at intervals of half an hour or an hour.

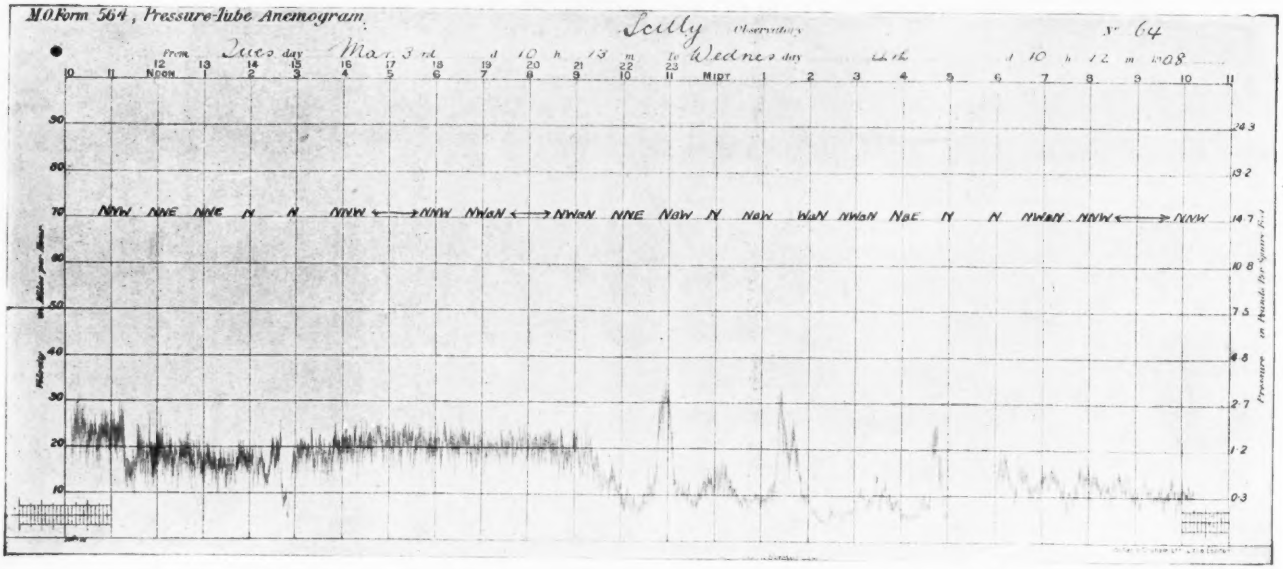

FIGURE 3. Recurring squalls at Scilly. Anemogram obtained at Scilly on March 3, 1908, showing a light wind about 10 miles per hour interrupted by occasional squalls reaching 35 miles per hour at 11 p.m., 33 miles per hour at 1.30 a.m., and 25 miles per hour at 4.45 a.m. The duration of these squalls is from 10 to 20 minutes.

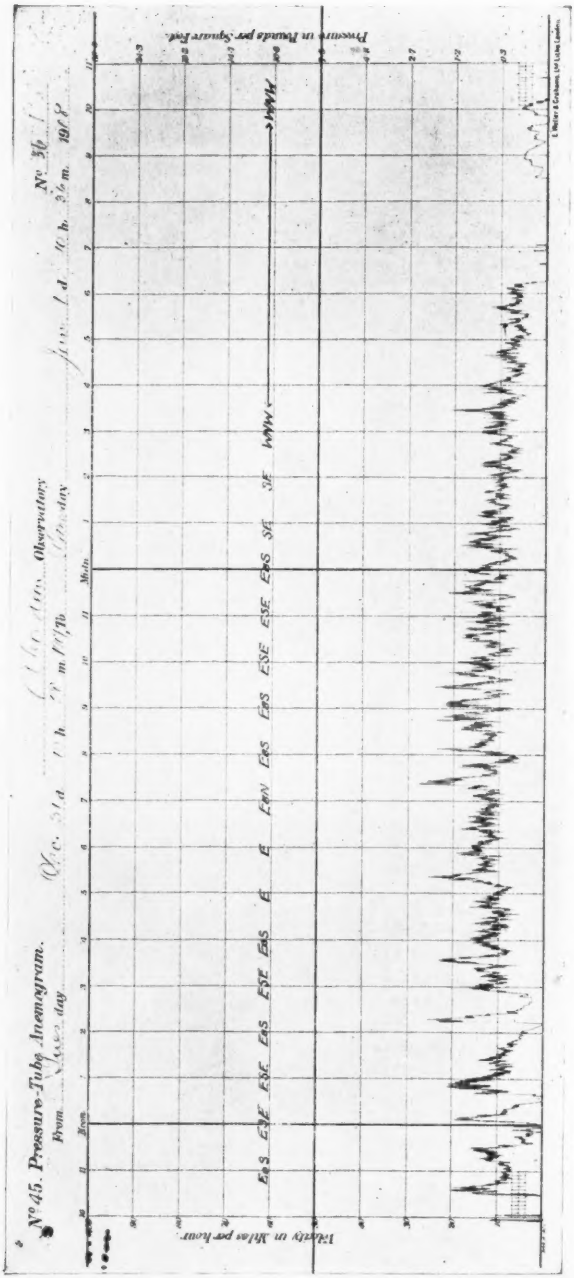

FIGURE 4. Recurring squalls at Aberdeen. An anemogram obtained at Aberdeen on January 1, 1908, showing a succession of twenty-seven "squalls" in a light wind between 10.30 a.m. December 31 and 6 a.m. January 1. The intensity of the squalls is not great, as the highest velocity is less than 30 miles per hour, but the squally fluctuations as distinct from the gustiness of short period are well marked.

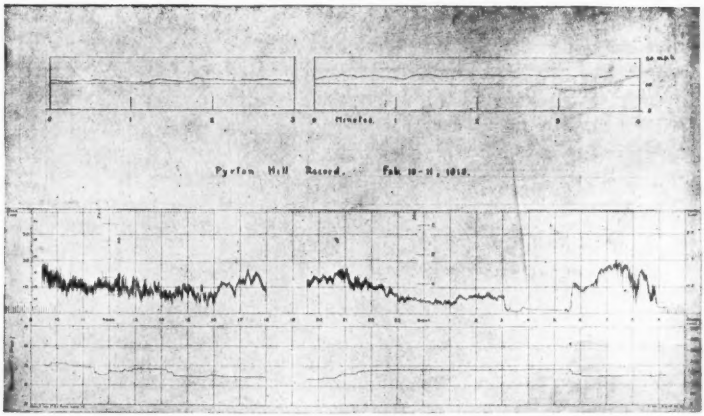

FIGURE 5. Examination of ordinary gustiness by "quickrun." The lower part of the diagram shows the ordinary trace of an anemogram (for velocity and direction) at Pyrton Hill. It is interrupted between 6 p.m. and 7.30 p.m., and in the interval a record of velocity with open time scale was obtained, specimens of which are shown in the upper part of the diagram, where the horizontal graduation is in minutes. The wind, about 10 miles per hour from S.E., is singularly free from gustiness.

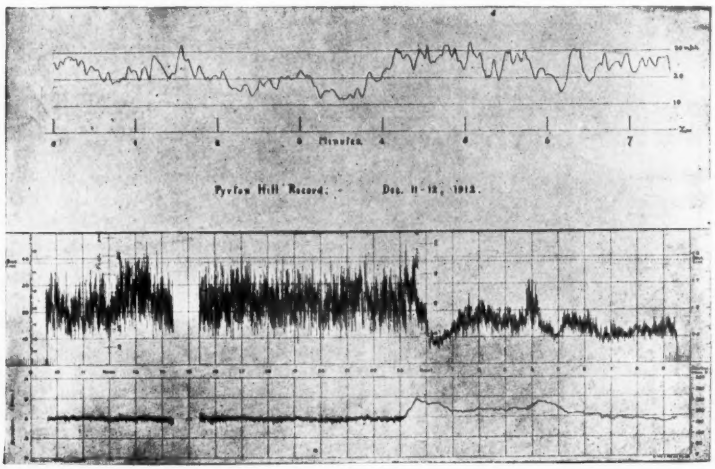

FIGURE 6. Examination of gustiness by "quickrun." In the lower part of the diagram is shown the Pyrton Hill record of anemograph for velocity and direction on December 11-12, 1912. It is interrupted from 2.25 p.m. to 3.20 p.m., and in the interval a record for seven minutes with an open time scale was obtained, which is shown in the upper part of the diagram. The trace shows the details of wind velocity in a moderately gusty south wind of 20 miles per hour, so far as they can be recorded with an instrument of the pressure-tube type. The question of the time constant of the instrument is dealt with elsewhere.

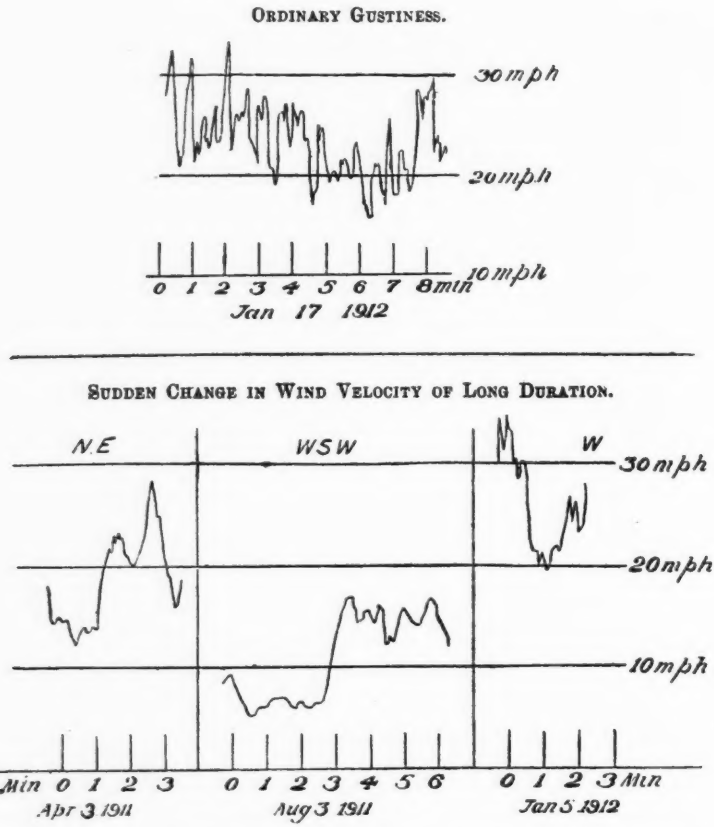

FIGURE 7. Different types of gustiness disclosed by examination with "quickrun."

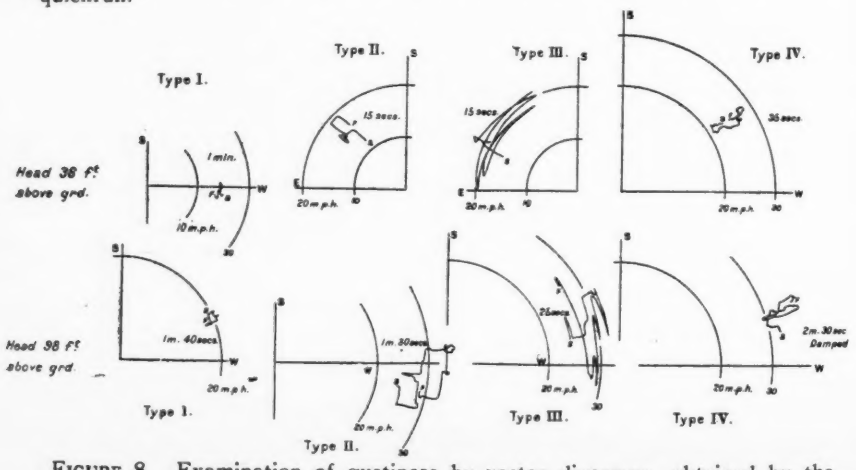

FIGURE 8. Examination of gustiness by vector diagrams, obtained by the

pressure-tube anemograph for direction and velocity, modified to show the vector-change in the wind. The writing point moves round with the veering and backing of the wind, and its distance from a centre indicates the velocity of the wind. The direction and velocity for any point of the trace is indicated by the line joining the point to the centre. A regular succession of similar vortices, with vertical axis, passing the anemometer, would be represented by the repeated tracing of a closed curve. The figure shows a number of examples indicating great irregularity in the corresponding changes of direction and velocity, and consequently the absence of any regular sequence of vortices with vertical axis. The time occupied in tracing the diagrams is indicated for each of the separate traces.

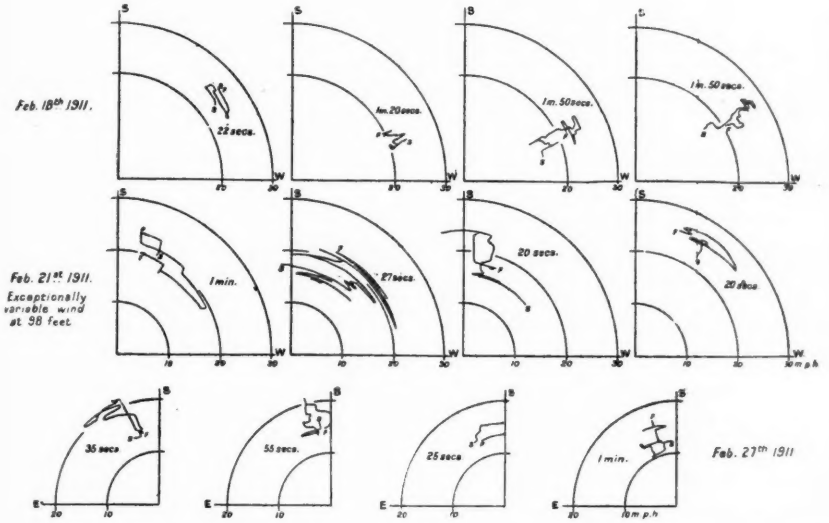

FIGURE 9. Further examples of the investigation of gustiness by vector diagrams of the wind, showing different types of structure, from the rounded figure indicating a horizontal vortex motion No. 2 to irregular fluctuations of direction with spasmodic changes of velocity No. 6.

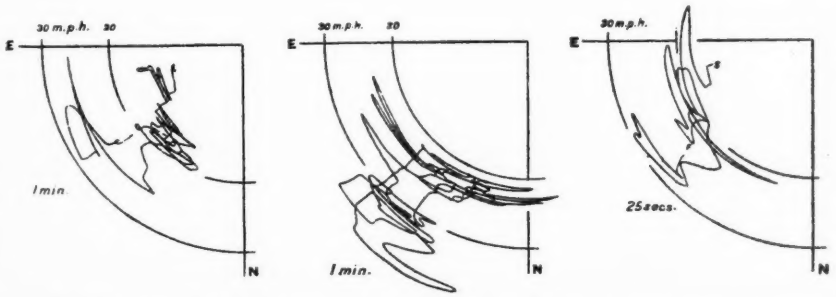

FIGURE 10. Further examples of the investigation of gustiness by vector diagrams of the wind. Three examples of conspicuous and independent fluctuation of direction and velocity in a N.E. wind at Pyrton Hill.

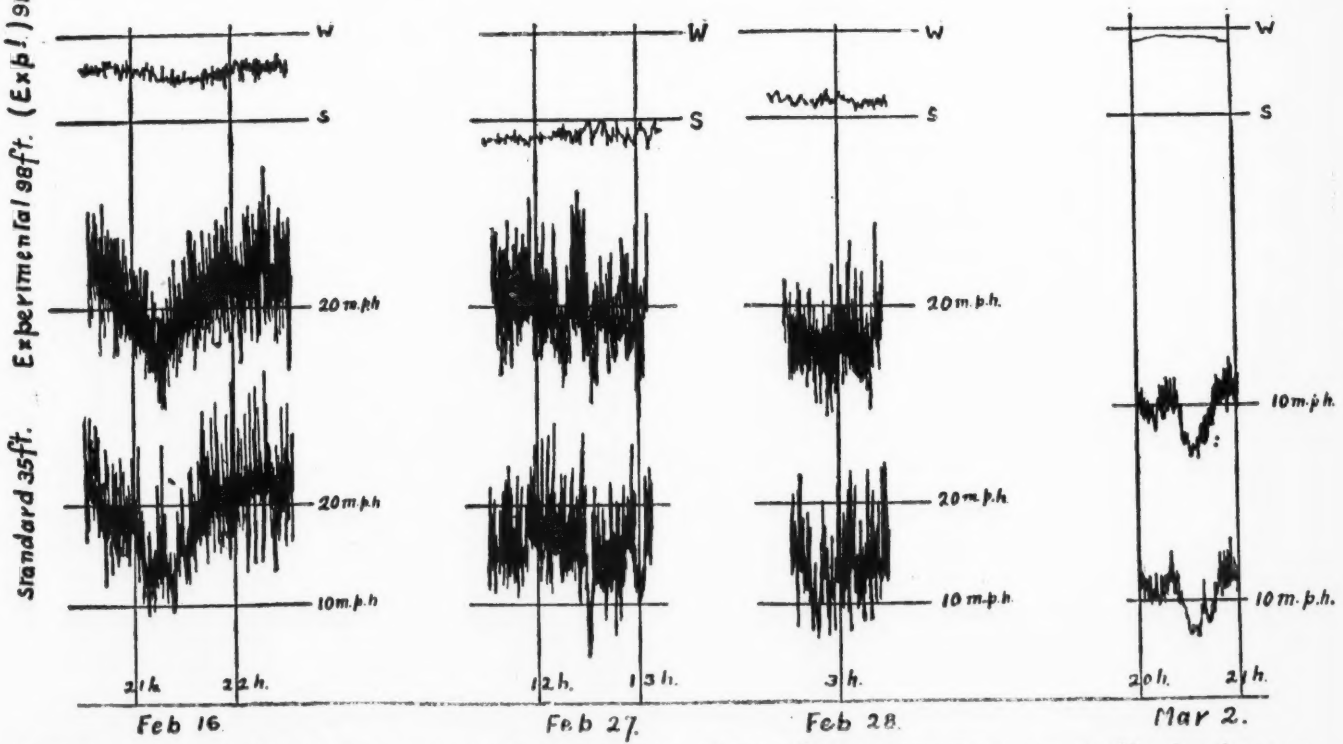

FIGURE 11. Examination of gusts and squalls by the traces of two adjacent anemometers, one at 98ft. elevation on a tower of open steel-work and the other at 35ft. on a house about 100 yards away. A comparison of the traces shows that, besides the general gustiness of the wind, which is only slightly dissimilar for the two exposures, there are squalls which are quite dissimilar in the two traces.

Gusts are fluctuations of wind force at short intervals; there is no precise period about them; there is every stage of gradation of them from what we have come to regard as the ordinary gustiness of an otherwise "steady" wind, with about seventeen fluctuations a minute, to the equally irregular "recurring squalls" themselves made up of groups of gusts at intervals of half an hour, more or less.

These phenomena may, I believe, be regarded as the turbulent motion of the atmosphere as distinguished from the stream line motion of the undisturbed atmospheric current, and by the turbulent motion I mean that which consists of "circulation" and which is illustrated and symbolised by the eddy, the whirlpool. the whirlwind. Strictly speaking, I suppose all air currents must have in them a certain amount of circulation in consequence of their movement being carried out on the rotating earth, but I am proposing for the present to leave out of account that part of the atmospheric motion which can be represented by the balance between the pressure gradient γ and opposite "acceleration" $2\omega V\rho \sin \lambda$, due to the angular rotation ω of the earth and to deal exclusively with that which has to do with rotation with comparatively small radius; anything, let us say, between 10ft. and 100 miles.

I exclude "eddies" with a greater radius than 100 miles, because the meteorological atmosphere comprising about three quarters of the whole mass is only about six miles thick, and consequently a hundred mile vortex cannot be much less flat than a penny. Now even in a penny (outside a slot) it is the image and superscription which are the important things, not the general shape; it is no longer the roundness and symmetry but the want of symmetry that tells us anything; and so with the discs of atmospheric vortices more than 100 miles in diameter, as shown on any weather map with a marked cyclonic disturbance, we cease to regard them as a single whole; the characteristics of the several parts then become the more appropriate subject of study. (See Figures 12, 13).

XUM

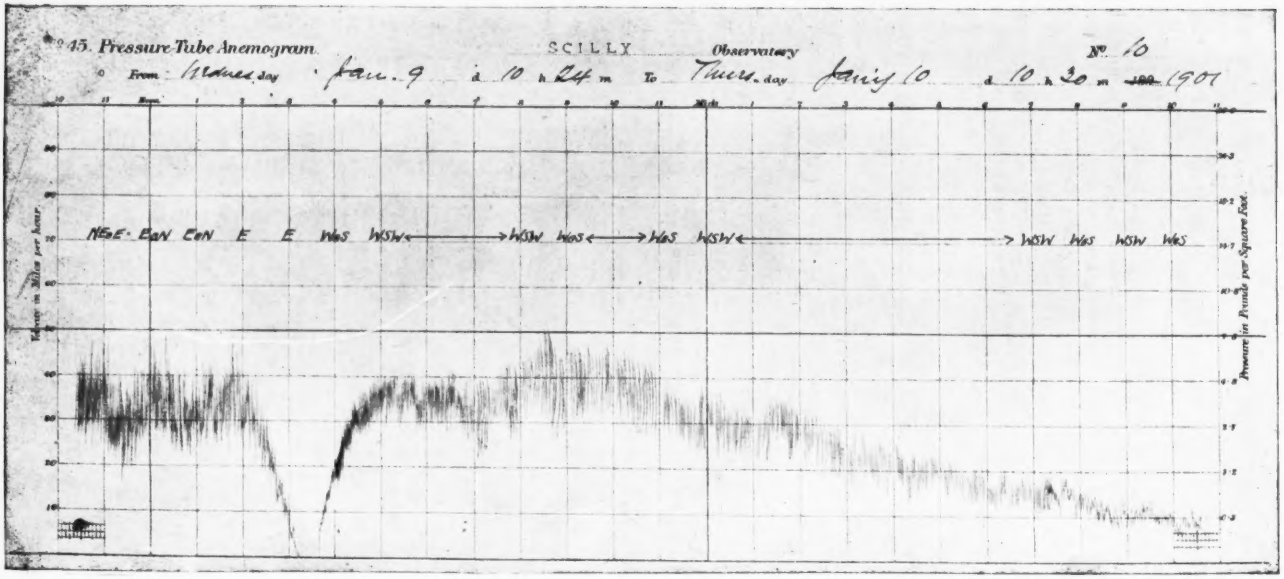
45. Pressure Tube Anemogram.
SCILLY
Observatory
No. 10
From Wednesday Jan. 9 à 10 h 24 m To Thurs. day Jany 10 à 10 h 30 m 1901
NNE. ENE ENE E E WbS WSW WSW WbS WbS WSW WSW WbS WbS WSW
Velocity in Miles per hour.
Pressure in Pounds per Square foot.

FIGURE 12. Anemograph record during the passage of a deep cyclonic depression with circular isobars over Scilly in the afternoon of January 9, 1901, showing a rapid lull in the wind velocity from an E. wind of 35 miles per hour to dead calm between 2 p.m. and 3.15 p.m., followed by an equally sudden rise from dead calm to 35 miles per hour from W.S.W. between 3.40 p.m. and 5 p.m. corresponding with the passage of the centre of the depression over the station from South to North.

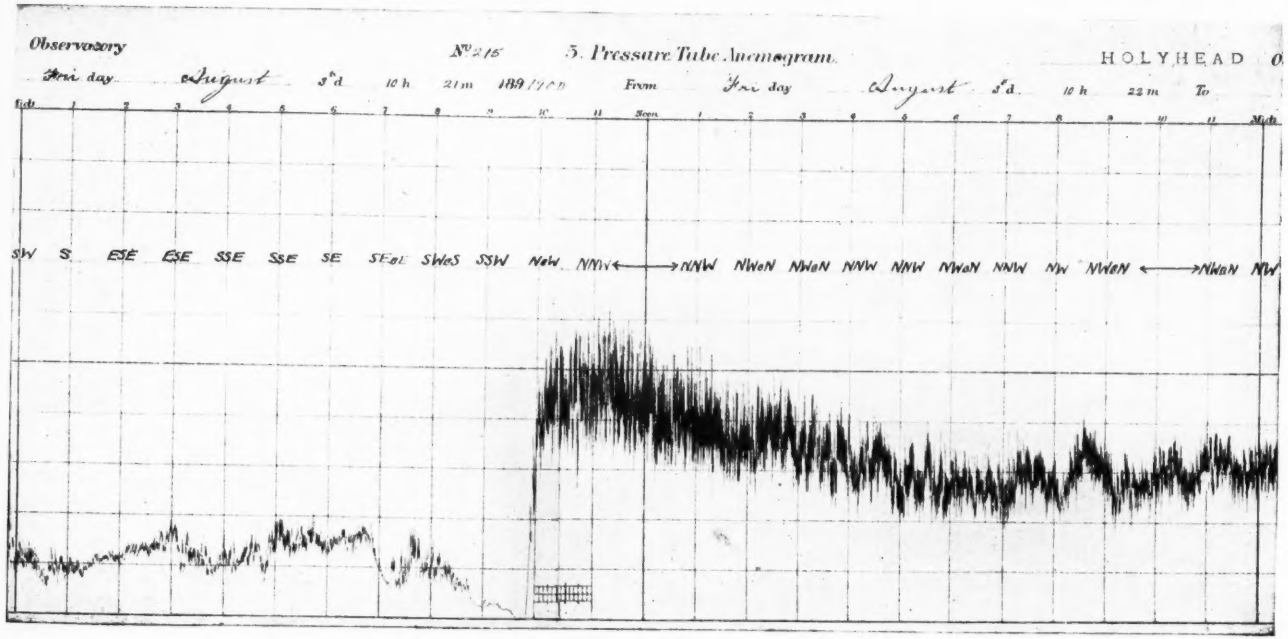
Observatory
No 215
5. Pressure Tube Anemogram.
HOLYHEAD
SW S ESE ESE SSE SSE SE SEbE SWbS SSW NbW NNW
NNW NWbN NWbN NNW NNW NWbN NNW NW NWbN
NWbN NW

FIGURE 13. Remarkable change in wind attending the passage of a cyclonic disturbance over Holyhead in the forenoon of August 2, 1900. The disturbance passed from N.W. to S.E., and until 7 a.m., while the wind was south-east, it was about 15 miles per hour. It veered rapidly to S.S.W., and fell calm at 9.35 a.m. At 9.50 it began to rise with great rapidity, and reached gale force from N. by W. in ten minutes. After a period of gale in which gusts of 60 miles per hour were recorded it settled down to a moderately steady blow from N.N.W.

Causes of Atmospheric Turbulence.

The question for which I ask your special attention this evening is the cause or causes of the turbulent motion which we find in the atmosphere. I do not propose to deal at length with the steady atmospheric currents which are directly related to the rotation of the earth; they form a subject which is interesting enough in itself, but is different from that which we are now dealing with, and we have a ready means of distinguishing the two when the scale is sufficiently large. In the cases which we are now considering the deviations from the straight or great-circle-path are counter-clockwise round a core of low pressure (in the Northern Hemisphere) and therefore in the opposite way from the deviation which would be produced by the undisturbed effect of the rotation of the earth. We have to deal in fact with local cores of low pressure maintained, if they are persistent, by a circulation round them.

Eddies and Gustiness due to Obstacles, Large or Small.

(Figures 14 to 17).

We know for certain from experiment, as well as from observation, that eddy motion is produced when a steady current of air moves along even a smooth, solid, or liquid surface, and still more so when it passes an obstacle, so that the surface of the sea, or of flat land, or still more conspicuous obstacles in the shape of waves, cliffs, buildings, trees or woods, will cause eddy motion. Much time and trouble has been spent at the National Physical Laboratory at Teddington in securing a current in an air channel reasonably free from eddy motion, and it is this kind of eddy motion which is manifest in the gustiness of an ordinary wind. We may be pretty sure of that from the differences which we notice between the gustiness of different exposures as recorded by our anemometers; and in illustration of this point let me call attention to the traces of the anemometers at Aberdeen, Pendennis Castle, Shoeburyness, and lastly Gibraltar. The gustiness is evidently dependent upon the direction from which the wind comes. The wind which advances over a gradual slope direct from the open sea is much less gusty than that which comes over land.

Moreover, the gustiness as estimated by the range of velocity during any interval to the mean velocity for that interval has been shown by examples to diminish with increasing height, but the law of decrease is evidently not a simple one.

The most extensive fluctuations in the wind are those which are shown on the anemometer at the top of the rock of Gibraltar when the wind blows on the east cliff, which is very abrupt, but in that case we have to deal with a special form of localised disturbance which we call the cliff eddy.

Permanent Eddies over Cliffs.

When the wind blows on the face of the cliff with sufficient velocity we get a permanent eddy which can be observed in a strong wind on any cliff, and which, in the case of Gibraltar, is bounded by a sheet of air going upwards at a steep angle sometimes for hundreds of feet. The peculiar characteristics of the anemometer trace in that case probably represent not so much fluctuations in the velocity of the wind itself as the fluctuations in the upward direction, to which the anemometer responds by the sudden transition from pressure to suction at the opening which is designed for pressure. As the variation of the upward direction depends upon the velocity with which the air approaches the cliff, we get the interesting feature of an optimum value for the wind velocity shown in the chart, which is not the maximum of the wind velocity.

This eddy motion of air flowing past obstacles is thus of a complicated character and it is a question whether we can describe it in general terms at all.

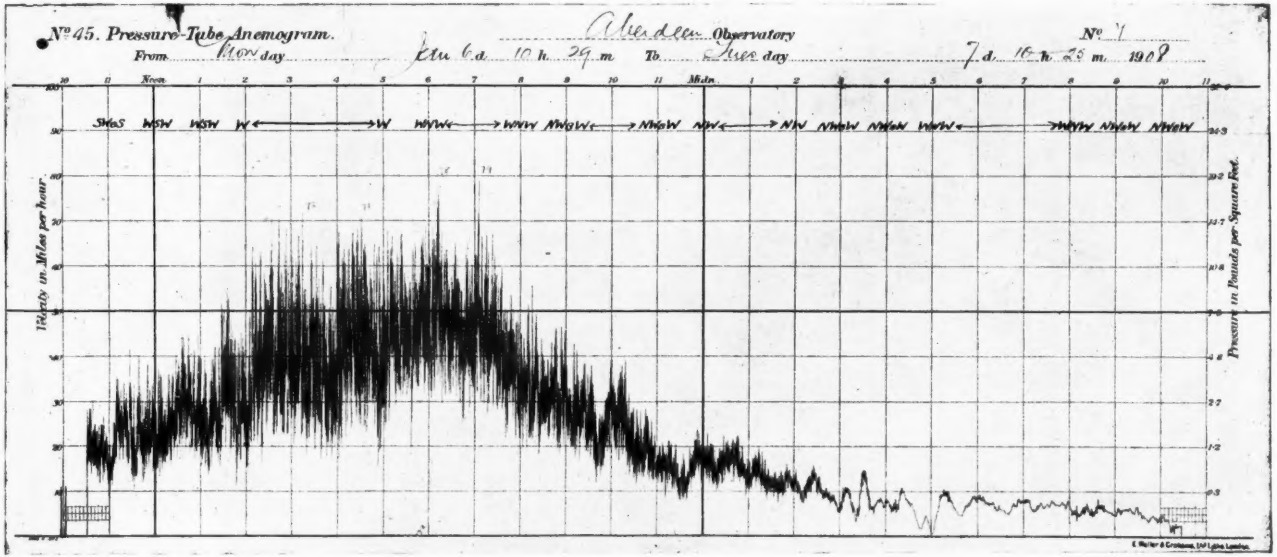
No 45. Pressure-Tube Anemogram.
Aberdeen Observatory
No 1
From Mon day Feb 6 d. 10 h. 29 m. To Tues day 7 d. 16 h. 25 m. 1909
Noon
Midn.
SSoS SSW WSW W WbyW WNW NWbyW NbyW NW NNW NNoW NWoW WNW NNW NWbyW
Velocity in Miles per hour.
Pressure in Pounds per Square Foot.

FIGURE 14. Trace of anemogram taken during the passage of a cyclonic depression over Aberdeen, showing the increasing range of gusts with increasing wind force. When the wind reaches a mean velocity of 40 miles per hour it fluctuates between gusts of 55 miles per hour and lulls of 25 miles per hour. The ratio of the width of the "ribbon" to the mean velocity is thus about 3/4. The exposure at Aberdeen is thus characterised by the large range of gusts.

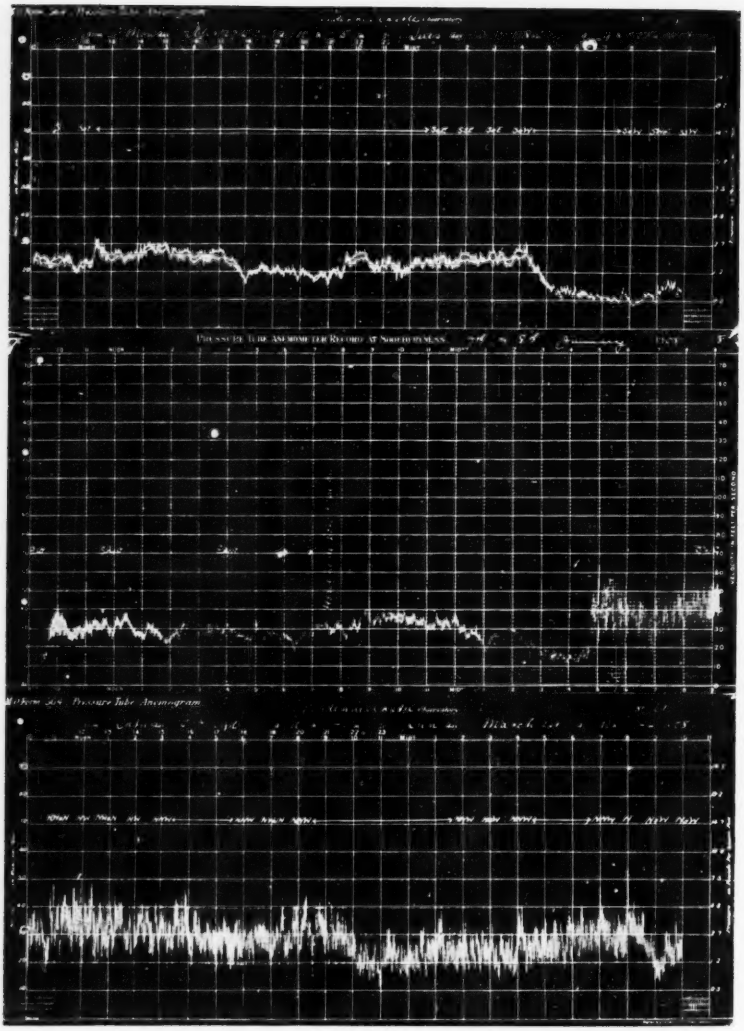

FIGURE 15. Three traces from anemograms. The first is from Pendennis Castle, and shows a singularly steady sea-wind from S.E. The range of gusts and lulls at 15 miles per hour is only about 4 miles, so that the ratio of range to mean velocity is 4/15, as compared with 3/4 at Aberdeen. The third trace is also from Pendennis, and shows considerably larger ratio of fluctuations, 15/35 in a N.W. wind which comes overland.

The second trace is from Shoeburyness, and shows a noteworthy increase of the gustiness there when the wind changes from S.S.W. to W.N.W.

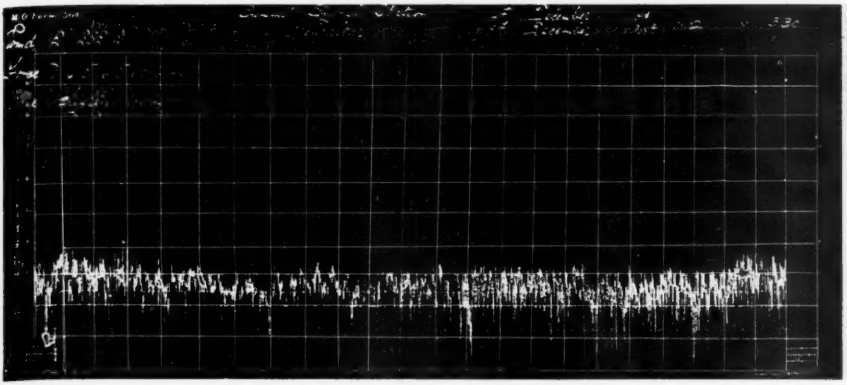

FIGURE 16. Trace from the anemometer on the top of Rock of Gibraltar. The trace shows numerous complete lulls, which may be attributed to the air passing the anemometer at so steep an angle that the wind causes suction instead of pressure upon the pressure-tube. The limitation of the recorded velocity, which is also characteristic of the trace, is also probably due to the increase in the steepness of the angle with increased velocity of the wind impinging on the cliff.

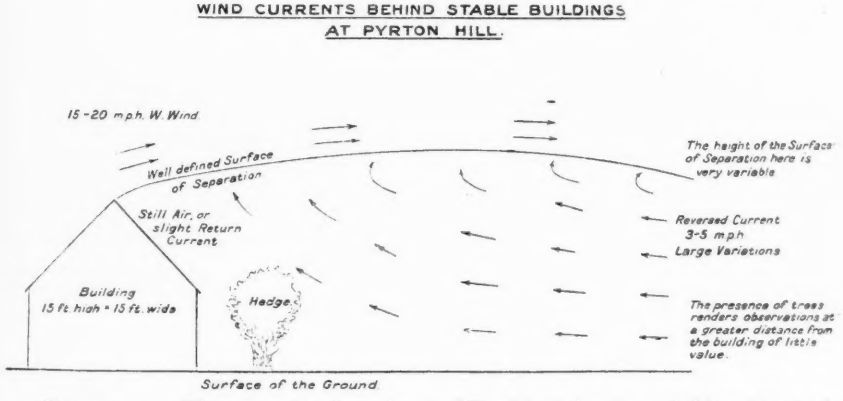
WIND CURRENTS BEHIND STABLE BUILDINGS AT PYRTON HILL.
15-20 m.p.h. W. Wind.
Well defined Surface of Separation
The height of the Surface of Separation here is very variable.
Still Air, or slight Return Current
Reversed Current 3-5 m.p.h.
Large Variations
Building 15 ft. high × 15 ft. wide
Hedge
The presence of trees renders observations at a greater distance from the building of little value.
Surface of the Ground.

FIGURE 17. Diagram showing the "cliff eddy" in the neighbourhood of a stable at Pyrton Hill, due to a 15-20 mile wind blowing on the side of the building.

Vortex Rings.

The case of eddy motion with which most people are familiar and which is most easily described is the vortex ring, which can easily be obtained by a pulse of air passing with suitable rapidity through a circular opening. In that case the eddy motion is quite persistent and the vortex ring travels with a velocity depending on its speed of rotation. If the vortex ring is circular it moves without change of shape; if it is not circular, but still a complete ring, it is still persistent, but oscillates about the mean position of circularity.

Incomplete Vortices.

The vortex ring must be complete in order that its core of low pressure may be completely protected. If the core is not protected the whole system rapidly disintegrates. We know from the persistence of semi-circular vortices in water that the core of a water vortex can be sufficiently protected by the creation of a gravitational pressure-difference at the cut end, and a smooth rigid surface would do as well; but in the atmosphere there are no facilities for those kinds of protection and therein we have consequently to deal with vortices necessarily incomplete and in all stages of disintegration. The complete circular or semi-circular persistent vortex is therefore of little help to us and we must seek some other mode of description.

Relation of Vortices to Gusts.

Experiments at the National Physical Laboratory have shown that a steady air current passing an obstacle throws off a succession of nearly complete vortices which are sufficiently persistent for their shape to be recognised, though subsequently they disintegrate rapidly. Out of the steady motion and the obstacle we get a sort of pulsating motion, which on the larger scale would be represented on an anemogram by a series of gusts. One step in the description of the turbulent motion, due to an obstacle, would therefore be to say how frequently these vortices are given off. I speak with diffidence for I do not know what the law is, but I suppose that with an increasing current the eddies are formed and sent off more and more rapidly until we get a quasi-permanent but pulsating cliff eddy from which masses of air in turbulent motion pass away to leeward.

Turbulence in Absence of Large Obstacles.

The eddies due to a comparatively flat surface of land or water are still more difficult to describe. Mr. Mallock has pointed out that the eddy motions in steamer smoke are due to wind and water and not to wind and steamer, and I have often watched steamer smoke with a view of resolving its convolutions into something which could be described, but so far without success. I suppose that for a wind of given velocity and a given surface a certain amount of turbulence is introduced which could be estimated numerically, and that the amount of turbulence is related to the gustiness shown on an anemometer record.

I hope shortly to have information that will throw some light on the question whether the surface turbulence is reduced or enhanced by flexible boundary conditions such as a grove of trees. Dr. J. E. Crombie, of Dyce, near Aberdeen, has been good enough to instal an anemometer with its vane projecting ten feet out of the top of a grove of trees on his estate, and I am looking forward to a comparison of the records, for summer with leaves on, and winter without leaves, with no little interest.

Turbulent Motion without Material Obstacles.

There is, however, another question to which I shall now ask your attention and that is whether material obstacles are necessary for the production of turbulent motion that may affect an airship or an aeroplane. To that I think the answer is obviously "no," and it leads to the consideration of the origin of turbulent motion in the free atmosphere itself. In the tropical revolving storms we have examples of real vortices with vertical axes maintained sometimes for several days together. They are a hundred miles or so in diameter, they travel at a slow rate, about 10 miles an hour, and they originate over the sea in circumstances which have little or nothing to do with surface friction. The difficulty about them is to understand how the low pressure at the core is maintained when all the processes which we see going on tend to reduce it. If that

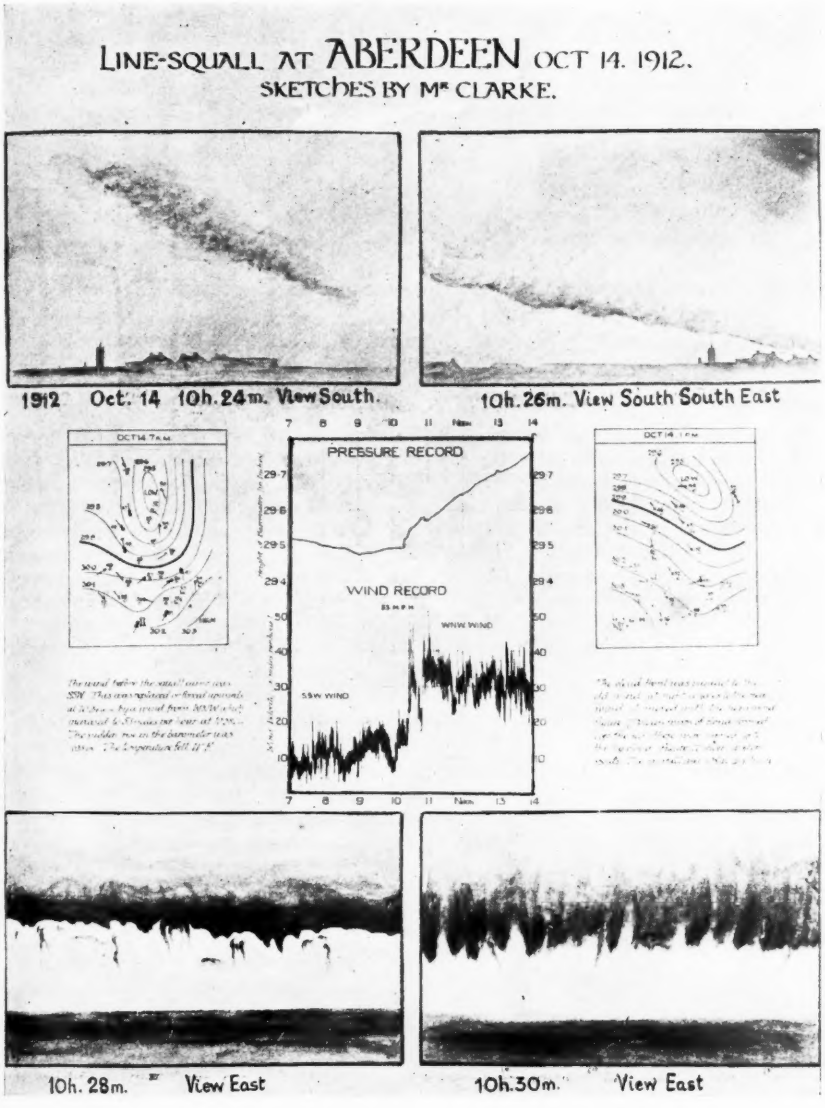

FIGURE 18. Six minutes in the life-history of a line squall of October 14, 1912. Photograph of collection of information about a line squall at Aberdeen, framed in the Meteorological Office, and comprising four sketches of the development of the cloud and subsequent rainstorm for intervals of two minutes, the barograph and anemograph records maps for 7 a.m. and 6 p.m., and notes on the changes of wind direction and temperature.

The wind was originally parallel to the line of cloud shown in the two upper sketches, and subsequently at right angles to the same line.

difficulty were disposed of the rest is easy; the conservation of angular momentum would account for the violence of the winds if the successive rings of the vortex are diminishing the diameter.

Besides these we have the tornadoes of the North American plains, which are perhaps a quarter of a mile wide and seem also to be rotary; they last for a few hours. And there are besides the occasional dust "devils" or little whirlwinds and waterspouts, which are similar but less destructive.

About these three types of turbulent motion let me remark that for some reason or other, which I cannot give you, they have the characteristic of persistence for days, hours, or minutes, as the case may be.

Turbulence set up by Temperature Differences.

Now there is a cause of local circulation in the atmosphere, in the sense of turbulent motion, in the convection of relatively warm and cold air. We seem to be able to trace cause and effect in this matter in the case of the line squall in which apparently there is a long line or cascade of descending air which pushes up the warmer air in front of it. In general character the phenomena are not, strictly speaking, vortex motion, because the air that descends, being cold, will not rise again and complete its circle, but there might very well be a core of turbulent motion between the two currents. There is also generally some horizontal circulation, because the two main currents are seldom parallel, so that it is quite possible, and even likely, that the violent relative motion may give rise to line eddies which ultimately drop one end on the earth. Mr. Clarke's sketches of the line squall of October 15th (Figure 18) seem to illustrate this process. They show the cloud formation, the turbulence with horizontal axis degenerating into little waterspouts of eddy motion with vertical axes. But the "where?" and "when?" of the formation of such vertical eddies out of the vertical relative motion are beyond our powers. Certain it is that the destruction of the Zeppelin off Heligoland, on September 8th last, may be attributed to turbulent motion originating in the convective condition of the atmosphere itself.

Disturbances due to Cloud Formation.

And here let me point out, in conclusion, that convection of any kind in the atmosphere must necessarily disturb the distribution of air currents and give rise to eddies and variations of wind such as those which we call squalls.

We are accustomed to regard the formation of cloud as carried on in the general air current without much disturbance of other atmospheric processes. I have been recently computing in some detail the changes of pressure-difference in different layers, and I have arrived at the formula

$$.0342\,\frac{p}{\theta}\left(\frac{\Delta\theta}{\theta}-\frac{\Delta p}{p}\right)$$

(where Δp is the difference of pressure between two places on the same level, and $\Delta\theta$ the corresponding difference of temperature) as expressing the increase in the pressure-difference between two places for a metre increase of height. With this formula applied to each successive metre we can find the pressure-difference at any level from the existing conditions of temperature. Now suppose a convective circulation to take place, with the deliberation that the atmosphere always shows, we get at once the essential elements of eddy motion.

Suppose that for a kilometre of thickness the air becomes five degrees warmer in the one vertical and five degrees colder in the other; the mean difference in

Δp may be left out for the moment, we get for the change introduced

$$34.2 \times 3 \times \frac{10}{300}$$
$$= 3.42 \ mb$$

The pressure difference at the bottom may be not more that 10 *mb* and, consequently, the result of the convection, which appears to the onlooker merely as a striking cloud, is also a signal to the bottom to change the velocity of the wind there by 30 per cent. and the wind pressure by about 60 per cent. So we must regard every warm patch and every cold patch in the upper air not merely as floating along and doing nothing, but as making up the pressure distribution upon which the surface wind depends, and every disturbance of temperature by convection means a corresponding disturbance of the regularity or steadiness of the wind.

When, therefore, one speaks of the structure of a disturbance for the purpose of aeronautical science, let it not be supposed that the actual wind-structure of the moment is the end of all things; the temperatures of the different parts of the structure are, at least, an important element in the matter of squalls and wind, and as a step towards the comprehension of the structure we must not overlook the temperature differences of different parts. Temperature differences cause convection and convection causes turbulence. Hence convective weather, whatever be its immediate cause, perhaps the sequence of events of a line squall or the intense local heating of a sunny day, is likely to be turbulent weather for the airman.

Figures 3 to 14 are reproduced by permission of the Controller of H.M. Stationery Office from the "Reports on Wind Structure" included in the reports of the Advisory Committee for Aeronautics.

DISCUSSION.

Mr. HARRIES *(communicated)*: With the rapid development of the practice of aviation, eddying movements in the atmosphere, hitherto regarded as of little or no moment, have assumed a position of very considerable importance. At first sight they may seem to have an independent existence, and to behave in the most disorderly manner, but no doubt the investigations which have been set on foot by Dr. Shaw will lead eventually to the discovery of the laws which govern their behaviour. Aviators will then know how to navigate their flying machines with confidence through what have thus far proved very troublesome and even dangerous difficulties. My experiments in the great eddies formed in the easterly wind current blowing across the Rock of Gibraltar were limited to two days, on the first of which the breeze was very light, and on the other it was strong to a gale in force. The observations were made at the Summit Signal Station, at a height of 1,300 feet above the sea, and immediately over the precipitous east face of the Rock (Fig. A). The results differed widely. In the weak current the toy balloons, and the bunches of white cotton wool and of black wadding were very constant in their behaviour (Fig. B). They passed quickly overhead to some distance above the west face of the Rock, then made a graceful turn downward and backward, and either returned to the starting point on the roof of the Station, or within a few yards of the west side of the building, the descent in the latter cases being along a line parallel with the edge of the cliff. In all cases the drifts were into the north-west quadrant, there was no instance of one passing to the southward of the Station. From Fig. C it will be seen that the captive balloons distributed about the signalling mast and the roof of the Station show the same circulating eddy at the lower levels, but up above there was clear evidence of the ascending air, the topmost balloon being elevated about 75° above the horizontal. On liberating, at intervals, the two topmost ones they ascended about 150 feet, passed away some distance to the westward, but finally returned, and came to

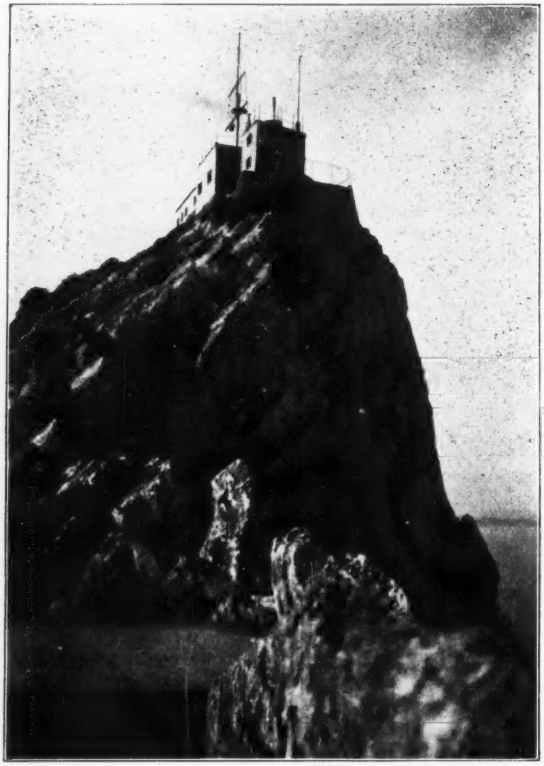

FIG. A.—SUMMIT SIGNAL STATION, GIBRALTAR, FROM SOUTH.

earth some 60 to 80 yards away to the north-west, on the line parallel to the cliff, already referred to. In Fig. D is represented the generalised results of the air movement in light breeze eddies. On the second day, when the anemometer registered a wind velocity of 30 to 39 miles an hour (the actual velocity was probably at least 50 miles an hour) nearly everything that was liberated was borne away towards the south-west and south. A few of the balloons shot up

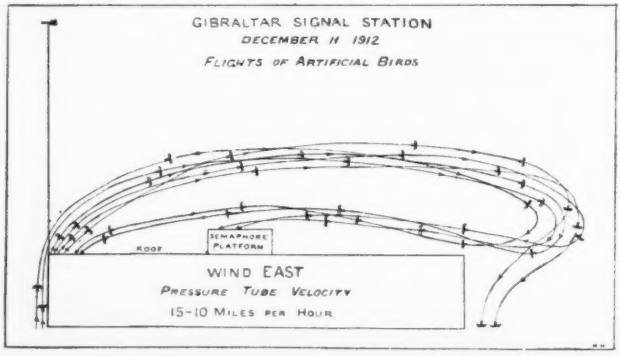

FIG. B.—FLIGHTS OF ARTIFICIAL BIRDS

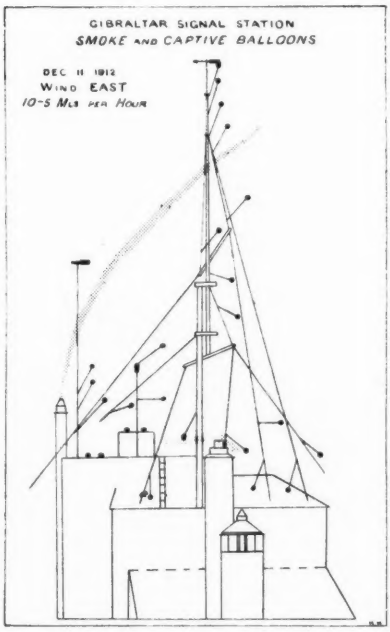

FIG. C.—SMOKE AND CAPTIVE BALLOONS.

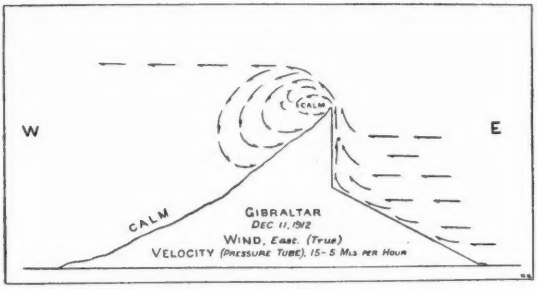

FIG. D.—LIGHT BREEZE CIRCULATION.

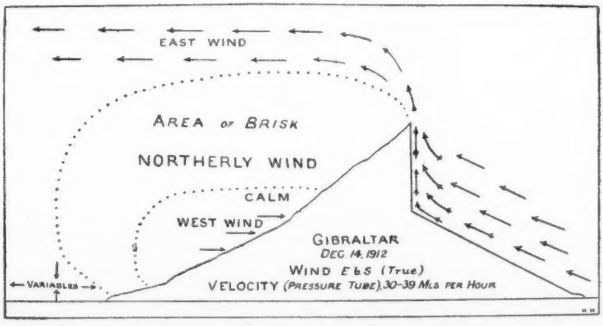

FIG. E.—STRONG WIND AND GALE CIRCULATION.

some hundreds of feet and attained the true wind current, travelling away across the northern portion of Gibraltar Bay. Those which did not mount 300 feet showed by their flights that a brisk Northerly wind extended from near the Signal Station westward out above the town of Gibraltar and the eastern side of the Bay. The balloons could be followed on their flights down to Europa Point and Carnero Point, the southern extremities (to east and west respectively) of the Bay. Fig. E shows that on this occasion there was a Westerly breeze low down on the west face of the Rock, a small space of calm separated it from the great area of Northerly wind, and above this again was the East wind. The Yeoman of Signals has since conducted experiments on the same lines, and obtained similar results. From an examination of the charts of atmospheric pressure for the several days of the experiments the conclusion arrived at is that the behaviour of the objects, whether they drifted towards north-west or towards south and south-west, was dependent upon the distribution of pressure. It is too early to make a definite assertion on the point, but clearly the eddies are under some control—they do not twist about in every direction without rhyme or reason. By continuing the observations at Gibraltar and elsewhere the correct solution of an interesting problem will be arrived at.

Major BROOKE-POPHAM: There is an idea very prevalent amongst pilots that it is easier to climb when flying against than when flying with the wind. An aeroplane will of course rise quicker in a given distance in the former case, but it also appears to rise quicker in a given time. I would suggest that this may be due to the difference in intensity of gusts when rising and when dying away.

Thus, suppose an aeroplane flying with an air speed of 50 m.p.h. head to a wind, the gusts of which are horizontal and increase rapidly, while dying away slowly. On meeting such a gust the aeroplane will for a short time be carried on by its own momentum, its air speed will be increased and it will rise. If the gust is of sufficiently long duration equilibrium will be obtained and the aeroplane will resume its air speed of 50 m.p.h.

On passing out of the gust and entering the part where the velocity of the wind is diminishing, the air speed of the aeroplane will fall and it will have a tendency to drop; but I suggest that if the velocity of the wind diminishes slowly, the power plant of the aeroplane will be able to accelerate it up to its normal air speed, before the force of gravity has time to act to any appreciable extent.

If, however, the aeroplane is flying with the wind, an opposite effect will be experienced and the net effect of the gust will be to make the aeroplane fall. Very probably my ideas will not stand the light of mathematical analysis and I regret that I am incapable of subjecting them to such a test myself, but I should be very glad to know the lecturer's opinion on the matter.

The lecturer referred to turbulent motion caused by wind in contact with the ground. This, presumably, occurs also, when two currents flowing in different directions pass over one another.

I may mention a case that occurred to me in December. On leaving Netheravon there was a cold north-west wind on the ground. It was quite steady up to 2,500ft. when it began to be bumpy. This state continued as far up as 2,800ft. or so, when the air got steady again and also much warmer. At the same time it was found that the aeroplane was drifting towards the north. Clouds were forming just below the 2,800ft. level and a mile or two further south was a layer of cloud between 2,000 and 2,500ft. up, about 50ft. thick, and, as was found later, extending as far as the sea coast.

It seems obvious that the eddies found in the layer 2,500-2,800ft. were caused by a warm southerly wind passing over the cold northerly wind and that as the former got cooled by contact with the latter, its moisture condensed and formed the clouds.

Doctor STANTON remarked that he was glad to find that Dr. Shaw had recognised the value of a more open time scale than that possible in the ordinary anemograph, for, as the lecturer had pointed out, the time duration of the average gust was only about 1/17th of a minute, so that it was obvious that nothing could be learned of the life history of a gust from records in which the time scale was two inches to the hour. In the recorder which the speaker had erected on the Tower Bridge the time scale was two inches to the minute and the history of the successive gusts could be traced with some accuracy. An interesting feature which he had noticed was, that on certain days when the phenomenon of recurring squalls was particularly marked, the time interval between successive squalls of widely differing intensities was of the order of the time interval between the successive gusts of the squall. This meant that changes of velocity in the wind such as, for instance, one from 20 miles per hour to 40 miles per hour could be experienced by an aeroplane in two or three seconds, which was of course a serious matter for the pilot.

Mr. C. J. P. CAVE showed a lantern slide of a photograph of some clouds which he thought showed indications of the wind being deflected upwards by hills.

Colonel H. E. RAWSON remarked that the best examples of gusts which he had ever experienced were those which occurred during the thunderstorms in South Africa. The contrast between the savage and short hurricane blast, before which eucalyptus trees 120ft. high bent as if they were blades of grass, and the equally short interval of perfect calm which followed, made this type of gust very noticeable. The strongest gust he knew of in this country registered 111 m.p.h., and these gusts must be not far behind in velocity. At a place where the full fury of the gusts is to be felt the clouds will give warning a couple of hours beforehand and white rocky cumulus will indicate convectional rising. But on the surface there will be nothing till 20 minutes before the burst occurs. Strong gusts and dust-eddies will make everyone out of doors seek for cover, the glass will rise with a jump and until the rain begins will fall and rise visibly. With the first drop of rain the glass will rise 0.04 inch and will continue to do so intermittently while the fury of the gusts lasts. This type of gust is what we usually call a squall and is certainly a turbulence set up by temperature differences. It is associated with eddy motion. On the western slopes of the Chiltern Hills anemometers have shown that easterly winds blowing over the hills give an uniformly high mean percentage of gustiness. Whatever direction the wind may be from a decrease of gustiness takes place with the altitude, and from 500 to 1,000ft. the mean gustiness has been found to be only 60 per cent. of the gustiness from 0 to 500ft. This type of gustiness was not considered at first, from the observations which were registered, to arise from eddy motion. He would be glad to know whether recent observations at Pyrton Hill have cleared up the point, or whether they still suggest some other cause than eddy motion for what is found to take place. In the case of the celebrated Helm Wind we have an east wind which blows down the western slopes of Cross Fell in Cumberland with hurricane force. But here there are no gusts but one continuous pressure, accompanied by a stupefying roar. The velocity rapidly decreases as the foot of the slope is left behind, and at the end of about half a mile there are nothing but light puffs coming from the opposite direction. This wind has been known to blow continuously for nine days and the winds in the district will then blow very irregularly, sometimes from the east and sometimes from the west. Any aeroplane coming into that area will certainly experience gusts for some miles round, for eddies of some intensity must be set up. They are not due to wind striking against an obstacle. The contour of the country shows that before rushing down the western slope with such violence the Helm Wind has passed for some distance over a high and rough tableland with dales and valleys without giving rise to any special phenomena. On arriving at the crest of the slope all the observations indicate that condensation of the aqueous vapour present in the atmosphere takes

place, and a veritable aerial cascade sweeps down the rough lee side of the ridge. The ground is then a curved solid boundary wall to the aerial stream and the velocity is greatest at some distance above it. Eddies must be formed there in such a case. If the fall of the slope was steep enough an eddy might be looked for on the surface of the ground and would be detected by a counter current. Nothing of the sort however occurs, and we learn from this fact that we must look to the contour of the ground when trying to locate the areas where eddy motion will give rise to gusts. The turbulence decreases with altitude and it is a sound rule for the aeronaut to follow to mount up higher when he finds himself in gusty surroundings due to obstacles.

Dr. E. H. HANKIN: The following facts indicate that wind variations cannot be fully explained in the light of existing knowledge.

(1) Both inland and at sea puffs* of wind occur during what otherwise appears to be complete or nearly complete calm. Those who have travelled to the East are aware that near the centre of the Red Sea an area of nearly complete calm usually exists, that the steamer takes one or two days to traverse. In this area the sea is flat and has a glassy surface. Here and there, over sharply defined areas, the surface is ruffled by minute ripples, due to very light puffs of wind. The steamer travelling at 16 knots (8.23 metres per second) may take from 10 to 100 or more seconds to pass through one of these "wind patches," thus giving an opportunity of estimating their extent. On one occasion (February, 1912) in the calm area I saw two butterflies fluttering along near the surface of the water. These butterflies were of small size and feeble flight. No land was in sight. But the lack of movement of smoke of steamers passing in different directions indicates the unlikelihood of wind near the surface of the water being the source of origin of the wind patches. That is to say, wind patches indicate puffs that cannot be due to obstacles causing an irregularity in the movement of the wind. In undisturbed weather in Agra in certain cases there is reason for believing that the wind falls to zero at night and that this calm extends up to cloud level (see my book, "Animal Flight," published by Iliffe and Sons, London, 1914, page 286). Every day in such weather the wind commences some time after sunrise, usually after soarability for cheels has been established, and before the air has become soarable for vultures. The first appreciable indication of the coming of the wind is the appearance of isolated and well marked puffs. Each puff may be seen moving leaves and small branches of the tree tops over an area that may measure from 20 to 50 metres in any direction. These early morning puffs often blow in widely different directions. I have seen one striking the lower and not the upper branches of a tree. At length slight wind is appreciable in the intervals between the puffs, and as the wind develops the separate puffs become less noticeable. Thus, in this case, apart from any question of explanation, it would seem more consonant with the facts to suppose that the wind was derived from the puffs rather than that the puffs were derived from the wind.†

(2) In the hotter and drier parts of India puffs in one wind may have little or no power of raising dust, while puffs in another wind may have a marked power of raising dust. The power of raising dust is not directly a function of

* The term "puff" is here used to designate an increase in the strength of the wind lasting more than a few seconds and of moderate intensity.

† Under the title, "A Peculiar Wind Effect," in the "Yachting Monthly" (London), Vol. XIV., p. 260, January, 1913, a discussion is given of evidence given by a photograph of the yacht "Skebenga" sailing with the lower sails showing wind from the port side, while the top sail was filled out as if by a wind from the starboard bow. This occurred in a deep-sea race at Durban, South Africa, for a Lipton Challenge Cup. The writer says, "One friend suggested a roll as the cause, but evidence of sufficient sea does not exist. Nor is there any likelihood of the yacht being by the lee . . . but all the sails look well filled and hard, even the jackyarder in its unnatural position."

the strength of the wind. Strong puffs in a strong wind may have no power of lifting dust, while lighter puffs in a lighter wind may have this power. But of two dust-raising winds it appears to be the rule that the stronger puffs or squalls raise most dust. The dust thus raised may either travel along in the form of an irregular sheet of wall whose surface is at right angles to the line of motion and in an apparently straight line, or parts of the mass of dust may exhibit a tendency to rotate. This tendency to form small whirlwinds is shown especially at the sides of a mass of dust. These whirlwinds originate after the dust has been raised. The rotation appears to be about as fast as could be given to the front wheel of a bicycle to which is given a single impulse with one hand. The rotation commences with almost instantaneous suddenness. No sign is ever seen of any inrushing draughts that might explain the rotation. If the wind is very light the tendency to rotate may be more marked and dust devils may be formed. The size and duration of dust devils is inversely proportional to the strength of the wind.

(3) Puffs of wind sometimes show a well-marked and remarkable connection with the appearance of transverse axis instability in certain soaring birds. During the puffs this form of instability is shown. Between the puffs the flight is stable. On other occasions transverse axis instability may occur without any recognisable connection with puffs. I have in my notes a case in which in a light wind this form of instability was well marked. On the preceding day in a strong wind no instability of this kind could be observed. In Agra in a puff in which birds show transverse axis instability no other form of instability is ever observed. A few observations made in Calcutta lead me to doubt whether in that place the different kinds of instability are so definitely separated as they are in Agra. Lateral instability and dorsal-ventral axis instability occur in Agra, especially when soarability is increasing or decreasing, and so far as I am aware have no connection with wind puffs. Current views of the nature of wind gusts appear to offer no explanation of these curious facts.

(4) Puffs of wind may be observed to aid soarability. In a soarable wind I have seen a cheel gliding up wind at slow speed. It was struck by a puff, as shown by movement of tree tops near. The speed of the cheel up wind relatively to the earth instantly increased. No change of wing disposition could be observed, and I believe that in this case none occurred. During the afternoon of the 22nd November, 1913, in Bombay Harbour, in full sunshine, and a light north-west wind making six-inch waves, the air appeared to be somewhat unsoarable, in that cheels, while ease-gliding, were making isolated flaps and usually were taking advantage of ascending currents on windward sides of ships at anchor. If they travelled up wind from one ship to another they had to flap the whole way. But I noticed that during a puff they were able to flex-glide horizontally up wind for any distance. When the steamer started, gulls arrived by flapping, and began gliding in the "soarable area" on the leeward side of the stern of the steamer. Other gulls remained gliding in the ascending current on the windward side of the steamer. I have reason for believing that, had the wind been fully soarable (for cheels), the gulls would have avoided this ascending current and would have kept to the soarable area to leeward of the stern (see "Animal Flight," page 257). The under sides of the wings of the gulls in the soarable area showed a faint dull yellow tint. The under sides of the wings of the gulls in the ascending current appeared white.

In certain winds the air is completely unsoarable. If these winds contain puffs, then certain puffs can yield energy for soaring flight and other puffs (which may be stronger) have no such power. It would be of interest to compare anemometer records taken in soarable and in unsoarable winds. Until this has been done, any opinion as to the nature of the difference between the two kinds of puffs would be premature.

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(5) Different puffs may produce widely different forms of ripples on the surface of the water. The effect of a moderate wind at sea on the surface of the water may roughly be described as being to produce several superposed wave forms. On the large rollers waves of from one to three feet in height are seen. Both the rollers and the waves appear to travel in the direction of the wind. The surface of the waves is covered with wavelets whose crests are sharp, branching, anastomosing, and irregular, thus causing the surface of the water to have a marbled appearance. So far as I am aware, these appearances are the same on different occasions, merely differing in size or relative size according to the strength of the wind and other known conditions. But during a puff or squall ripples are superposed on the other forms of wave motion, and may have widely different appearances on different occasions. If a squall develops, the surface of the water appears darkened, owing to the appearance of these ripples. These, when they first appear, at least, may be seen to have rounded tops. They lie side by side, and have some resemblance to ripple marks on sand. The distance from the crest of one ripple to the crest of the next appears (in my limited experience) to be from two to three inches. I have seen such ripples in a squall in the Arabian Sea which was strong enough to blow spray on board and also in a wind patch in the calm of the Red Sea. The wave length in each case, if my recollection is to be trusted, appeared to be much the same. But during a recent voyage from Bombay to Marseilles (22nd November to 6th December, 1913), during the four days' passage of the Arabian Sea and the first two days up the Red Sea, all the puffs or squalls observed were indicated by ripples of much smaller wave length than usual. As a rule, the separate ripples could only be distinguished by means of a binocular. In this wind flying-fishes flew badly. Their flights were always short, and usually very short. They showed two forms of lateral instability, variations of speed, height above the water, and wing disposition during the same flight. In one case, in a flight of about 50 metres' length, height was gained and lost several times before the animal finally plunged into the water. At length (on the 29th November), after passing through an area of the sea having a glassy surface, the steamer reached a wind patch having ripples of the ordinary size. On the margin of this wind patch a flying-fish was observed making a long and regular flight at uniform height above the water. On reaching the Mediterranean (on the 2nd December) still another arrangement of ripples was observed. I noted that the surface of the sea had a "crêpe-like" appearance during squalls, apparently due to isolated ripples of different sizes. The appearance resembled what one might imagine would be caused by the falling of small hailstones with larger hailstones at somewhat regular intervals. Neither rain nor hail was falling at the time. These squalls often covered large areas, a square mile or more, and through which the ship was occasionally passing.

Rarely in my experience have two sets of ripples been observed together at the same time. These occasions were: Red Sea, 16th February, 1912, all day; 20th February, 1912, Arabian Sea, at 11.22 a.m.; and Sunderbuns (Gangetic Delta), 21st October, 1913, at 6.50 a.m. In each case this arrangement was observed in wind patches in calm areas. The ripples were not placed closely side by side like sand ripples. They were isolated, and apparently distributed somewhat irregularly over the glassy surface of the water in two sets (Fig. 1). In one set each ripple was curved. When seen in plan, its shape was that of a crescent. In addition to these curved ripples of Series I., there was another set of ripples which were straight or nearly straight. These ripples of Series II. were set at an angle, generally about 45°, sometimes nearly 90°, with the curved ripples of Series I. The straight ripples of Series II. were placed parallel to each other at a distance apart of one or two feet. They apparently were of indefinite length.

It has been suggested to me that one of these sets of ripples was due to a

down draught or eddy reflected down from the ship. This was certainly not the case, for the following reasons :—(i.) Ripples of both series appeared to be of the same strength on both sides of the ship. (ii.) On each side they were of the same strength, so far as could be seen through a binocular, both near and far from the ship. (iii.) Eddies reflected down from the ship are known to me, and are of an entirely different shape. They only occur in the presence of an

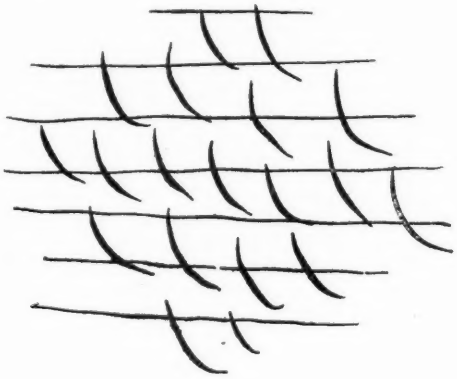

FIG. 1.

appreciable amount of wind. They are especially strongly marked on the leeward side of the "aft well." In a stormy wind abeam they may make a patch on the water one or two hundred metres to leeward of the ship. In the above observations eddies of Series I. and Series II. were nearly always equally distributed in each wind patch in which both kinds were present. At the margins of the wind patches both sets of wind ripples died away together. At 3.5 p.m. and 4.38 p.m. on the 16th February, 1912, I observed an appearance that suggested that three sets of ripples were present together (see Fig. 2). But in

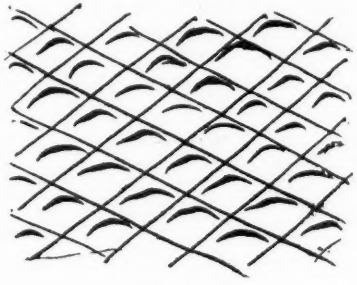

FIG. 2.

this case it was not possible to be certain that the ripples of Series II. and III. were not merely prolongations of the ends of the crescents of the Series I. ripples.

Vaughan Cornish, in a description of tidal ripples in sand formed under water, has described a case in which two sets of ripples existed at right angles to each other. He states that one set of ripples had been formed by one current and that the second set were formed when the current of water suddenly and completely changed its direction. (See "On the formation of wave surfaces in

sand," "Scottish Geographical Magazine," January, 1901.) If, following Vaughan Cornish, we attempt to explain the double series of ripples described above as being caused by successive action of two air currents, we must obviously suppose that these two currents were very different in their nature. It is obviously a matter in which any attempt at an explanation at present is premature. §

Lieut. HOLT: It has been remarked that aviators could greatly assist meteorologists, but I think that the latter could also assist the former. What is the meteorological difference between a remous and a gust? Does the word "remous" imply a vertical disturbance due to thermal changes, and "a gust" a horizontal disturbance due to a wind velocity change? It would seem that disturbances are often due to narrow horizontal waves or vertical movements, as one often meets a strong upward or downward movement, then three or four seconds of calm, and then an opposite movement of equal intensity to the first movement.

Most aviators seem to agree that in winter, at 2,000 feet over ordinary country, one is in the gradient wind, and is not affected by ground disturbances. In summer these disturbances sometimes extend up to 3,500 feet.

Further, if cumulus clouds are merely the condensed tops of upward currents, why is the air sometimes quite calm just beneath these clouds?

Major W. S. BRANCKER said that, speaking from a comparatively small experience, he thought that probably the sun gave the aviator more trouble in the way of gusts and remous than the wind.

Dr Hankin had given them a very vivid description of some rather alarming aerial disturbances prevalent in India even in light winds, and he (Major Brancker) concluded that these were the direct outcome of the great heat of the Indian sun. He had had the opportunity of making a few flights in India about three years ago, and of observing others, and, although ideal conditions on the ground were awaited before going up, the air was almost always considerably disturbed at a thousand feet or thereabouts. This was with a bright sun in the cold weather; in the hot weather the air would probably be very much disturbed, and they might hope to obtain interesting information on this point from the officers of the Indian Flying Corps when they started work.

As a soldier, he could say that military aviators were very anxious to learn all they could about meteorology. They knew very little about the science, and felt that it would probably be of very great assistance to them in the future, particularly in the matter of forecasting the approach of bad flying weather.

As a lately joined member of both societies, he would like to suggest that Meteorology and Aviation should advance hand in hand in the future; the Meteorological Society could be of great assistance to the aviator, and in the future, when the aviator knew more about meteorology, he could systematically record his air experiences from day to day, and so furnish the meteorological experts with a great deal of instructive information, which they could not obtain otherwise.

The lecturer had told them a good deal concerning air vortices, and there was one question on this point that he would like to ask him: Did he think it possible to create an artificial air vortex by means of explosives on the ground, which would seriously disturb the equilibrium of an aeroplane or airship flying at two or three thousand feet?

Mr. E. GOLD made some remarks with reference to the question of easier

§ A description of ripples is given by J. Russel Scott in "The Wave of Translation" (London: Trubner and Co., 1885).

climbing against the wind, on the importance of considering the vertical as well as the horizontal component of velocity in a gust. If no change of direction took place, the vertical motion in front of a gust would be upwards, while behind the gust it would be downwards. (Mr. Gold has since sent the following supplementary note):

Major Brooke Popham referred to the experience of aviators who found it easier to climb against the wind than with it, and I suggested an explanation which, upon closer examination, I found to be insufficient. In discussing the question with Mr. G. I. Taylor, he suggested that the cause might be the change in velocity with height, and this appears to furnish a reasonable explanation. It is well known that in general the velocity of the wind increases with altitude, so that when an aviator is climbing against the wind the effect upon the aeroplane is similar to that produced in front of a gust, and he is able to avail himself of the lifting power of this increasing relative wind in a manner analogous to that described by Langley in his paper on the "Internal work of the wind." If, on the other hand, the aviator is climbing *with* the wind, he is continually advancing into a region where the relative wind is decreasing, and he must expend some of the work done by his engine in making up this decrease if he is to keep up the lifting force.

Dr. A. P. THURSTON (*communicated*): Will Dr. Shaw be so good as to give us the benefit of his knowledge with regard to those aerial gusts or disturbances which are popularly known as "air-holes"?

REPLY TO DISCUSSION.

Dr. SHAW: Seven or eight questions have been raised in the discussion. About one-half of them turn upon the precise meaning of the words employed to describe the facts. Colonel Rawson remarks that a certain type of gust is "what we call a squall," and is certainly turbulence set up by temperature difference. It is associated with eddy motion, and he asks whether ordinary gustiness is now attributed to eddy motion. Dr. Hankin describes what a sailor would call "cat's paws," and, grouping them with similar phenomena of more intense character experienced in India, calls any such temporary and local wind a "puff." Lieutenant Holt suggests a distinction between "remous" as the result of thermal disturbance and "gust" as a mechanical phenomenon.

It will be best if I begin with Colonel Rawson's question as to whether gustiness is eddy motion, and so explain as I go on the mental picture that I form of the structure of the wind and the sense in which I have used the various terms. My habit in such matters is necessarily influenced by the constant use of weather maps. Everything in the experience of weather has to meet the question, Is it, or can it, be represented on the map? Now, cyclonic motion is represented on the map, but the turbulent motion of the air, the eddy motion, is not. In most of its manifestations its dimensions are altogether too small for mapping, and yet there is presumably no hard and fast line to be drawn between the "cat's paw" that just ruffles the surface of the water, the stronger "puff" of sunny countries, the local down-rush of the thunderstorm, and the great quasi-eddy motion with horizontal axis of the line squall, which can at least be indicated on a weather map by discontinuity in the isobars. In my vocabulary these are all "squalls." I think of them as winds which are temporary in a particular locality, because they represent some alteration in the vertical distribution of air currents that may take place in the free air chiefly on account of thermal conditions.

This kind of motion may give rise to local eddies with vertical axes. The final stage of the line squall represented in Mr. Clarke's sketches (Fig. 18) shows

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little waterspouts descending from the rain-cloud, and these represent real eddy-motion derived from the quasi-eddy motion of the line squall. There is, again, no line to be drawn between the funnel-shaped eddies below the rain-cloud and the all-powerful eddy of the tropical revolving storm which could certainly be represented on a map. So in our own country when an eddy reaches the dimensions of a tornado it can be shown on a map. It does not follow that it always will be shown there, the details which mark it are often overlooked. Besides these eddy motions which may have a thermal origin, or at least may originate in the free atmosphere itself, we have the eddies due to the effect of obstacles upon the wind. The wind itself can easily be represented on the map, the eddy motion cannot. Round every obstacle the air rolls as the smoke rolls over the edge of the steamer funnel. Mr. Harries' description of the effect of the Gibraltar Rock is quite *apropos*. The rolling motion of the air is the eddy motion of an incomplete vortex-ring caused mechanically. This expresses itself in fluctuations of the wind, which we call "gusts," and which last only until the air has rolled on. So far, our endeavour to identify the rolling character of the gustiness of wind has not been successful, but we have dealt only with the horizontal component of the wind. We have still to devise suitable means for examining the vertical motion in which the evidence of rolling is likely to be more easily identified. If that succeeds, we shall next try to find out the dimensions of the rolls, and so answer the question whether the turbulent motion near the ground is a single layer of eddies or a succession of layers.

Dr. Stanton has remarked that the time required for a squall to establish itself is of the same order as that required for a gust, so that one cannot tell by looking at an anemometer, when a rise of velocity begins, whether it is going to be a gust or a squall, and so in common practice a squally wind and a gusty wind are often synonymous; yet the difference between the two is a real one which meteorologists would like to have stereotyped.

A "squall" is a rapid increase of wind velocity that remains for over half a minute at least; it may include a number of gusts of different force. A "puff" or "cat's paw" is a squall of slight intensity.

A "gust" is an increase of wind velocity followed by a lull in less than half a minute; gusts are incidental to all winds, and depend in any particular case on the exposure of the anemometer by which the wind is registered.

I am not sufficiently sure of the acceptation of the term "remous" to say whether its origin is to be attributed to the free air or the surface. It may be a rising current which could be due to either, and not a change of horizontal velocity.

Colonel Rawson's description of the Helm wind makes it almost imperative that he should try his luck there with pilot balloons; it would be a most attractive holiday task to find out the actual distribution of the currents.

One other point may be briefly answered. Efforts to convert explosives into vortex motion with a view to affecting clouds have been pursued with great activity in the campaign of "weather shooting" in Southern Europe, but hardly with sufficient success to affect aeronautics. And still another point, the better climbing against the wind, is already disposed of by Mr. Taylor.

Major Brooke-Popham's description of the case in which a bumpy region was found at about the cloud level between a cold, steady north-westerly wind at the surface, and a warmer, but also steady, southerly wind at about 2,800 feet, is a most attractive invitation to complete an investigation which has been only just out of our reach for a long time, and that is the cloud-sheet with different winds above and below. We know that on quite a large number of days a cloud sheet may maintain itself for hours, always rather threatening, but without

actually lapsing into rain and without resolving itself into clear sky. The clouds which are visible move *with the lower current,* and at the top of the cloud layer (as above fogs and clouds generally) we may suppose the air gets warmer as one ascends. This inversion layer prevents any serious encroachment of the lower layer upon the upper layer, and thus introduces an element of stability not easily overcome; so that the whole process has a tendency to permanence. The most interesting region is the region of transition between the lower wind and upper wind where the clouds are formed by convection in the lower layer.

I should doubt if this state of things could give rise to turbulent motion of the kind which arises from an obstacle in the way of a surface current, but certainly the practical effect may be very much the same. We may picture to ourselves two wide sheets of air, many hundred miles wide, flowing in different directions and separated by half a mile or a mile of a transitional region.

Each current has its own local irregularities of warmth or of local disturbances, and these local irregularities are crossing each other's paths. It would require an extraordinarily light and airy tread for the upper current to carry its own irregularities without affecting its neighbour underneath or being affected by those below; but what the actual influence would be has not been made out. Certain it is that whenever vertical motion takes place there must be turbulent motion, because an ascending current implies a descending current, and these, with the two currents in different directions, complete the circulation.

It would be of great service if all such cases might be carefully logged by airmen for the information of meteorologists. This is evidently one of the many promising opportunities for co-operation between airmen and meteorologists. Others will certainly disclose themselves as we go on.

The CHAIRMAN, in proposing a very hearty vote of thanks to the lecturer, referred to the pleasure of seeing so well attended a joint meeting of the two Societies. He hoped that it was only a forerunner of many in the future. The question of joint action between the two Societies, mentioned by Major Brancker, was most important, and had received much attention from the Council of the Aeronautical Society. There was every possibility of something being done in that direction before long. (Applause.)

The meeting then adjourned to the anteroom for refreshments, and afterwards dispersed.

NOTE.—The blocks from which Mr. Harries's remarks have been illustrated have been kindly lent by the Council of the Royal Meteorological Society.

The NINTH MEETING of the FORTY-NINTH SESSION of the Aëronautical Society of Great Britain was held at the Royal United Service Institution, Whitehall, S.W., on Wednesday, 18th March, 1914, at 8.30 p.m. Major-General R. M. Ruck, C.B., R.E. (retd.), presided.

The CHAIRMAN: Colonel Holden, a Fellow of our Society and Chairman of the Accidents Investigation Committee, has kindly agreed to give us a lecture this evening upon the Investigation of Accidents, a subject of vital importance to all of us and one specially before our minds at the present time owing to the lamentable loss of three gallant officers of the Royal Flying Corps during the last fortnight. The subject is many-sided and I hope those taking part in the discussion will treat it from all points of view. You will notice that Colonel Holden has arranged his matter in so admirable a manner that it will be easy for speakers to take up the different points in consecutive order and so greatly facilitate the discussion and reply.

LESSONS ACCIDENTS HAVE TAUGHT.

BY COLONEL H. C. L. HOLDEN, C.B., F.R.S., FELLOW.

When I was asked some six months ago to contribute a paper to this Society dealing with the lessons to be learnt from the study of the causes of accidents when flying, I agreed to do so because it seemed to me that a discussion upon this subject could not fail to be of interest and advantage to pilots, constructors and others by bringing before them in collective form the accidents, preventable and otherwise, which have happened, and in particular those which have been investigated by the Accidents Investigation Committee of the Royal Aero Club, on which your Chairman of Council, General Ruck, and several other Fellows and Members of the Aeronautical Society are very useful workers.

The Work of the Accidents Investigation Committee.

I would here remark that the Accidents Investigation Committee was formed in the early part of 1912 and that the first accident it inquired into was one which happened at the Brooklands Aerodrome on May 13 in that year. Since that time, and up to the present date, eighteen other fatal accidents—one of which was an accident to a spectator—have been investigated, and reports upon them issued by the Royal Aero Club. With a view of obtaining as wide publicity as possible, these reports have been sent to, and published by, the technical and lay Press; and, moreover, copies have been supplied to the Home Office, Admiralty and War Office, at the request of these departments of the State, all of whom have, in one way or another, rendered the Committee great assistance, and upon more than one occasion have expressed their appreciation of its work.

The Collection of Data after Accidents.

It may be opportune at this point to impress upon my audience the difficulty that exists in ascertaining the exact causes of accidents to aircraft. Where the accident is immediately attended with fatal results, it is obviously impossible to obtain the evidence of the principal actor or actors in the tragedy, and even where the accident is not fatal, but only results in serious injury, as

is well known, there is generally a loss of memory on the part of the victim, not only of what occurred after the accident, but of what took place before it, which makes such evidence very unreliable. Again, the evidence of spectators has to be sifted with the very greatest care and caution. The appearance of an aircraft in the air is, from its nature, deceptive, and its exact aspect, position, height and direction of movement is very difficult to appreciate, even to the most expert in such matters, and in very few cases indeed can conclusive direct evidence be obtained. The evidence of the unskilled spectator is more often than not obviously incorrect and worthless. It is of prime importance, too, that when an accident occurs, the portions of the wreckage should only be disturbed so far as to allow any human beings to be removed, and even then any portions disturbed from their places should be as soon as possible restored to their original positions. The great importance of this matter has been recognised by the authorities at the Home Office, who have issued instructions to the police throughout the country that in the event of an aircraft accident occurring, the wreckage and site are to be guarded and no interference permitted until the Accidents Investigation Committee or its representatives have examined it.

The Scope of the Paper.

I have not attempted in this paper to discuss the causes of all the accidents which have occurred since the art of flying was first discovered, or even the accidents which have occurred all over the world since flying became a practical possibility and comparatively common, for the reason that in only a few isolated cases has any complete investigation been made as to the cause of any accident in this or any other country until the Accidents Investigation Committee commenced its work. Apart from this, I think I shall be able to show satisfactorily that the causes which have originated the accidents which have been inquired into by the above Committee happen to cover the whole ground as far as one can judge.

The Classes of Accidents.

The causes of accidents may be divided into two classes :—

(A) Those due to the pilot.

(B) Those due to faulty design or to mechanical failure of some portion of the aircraft.

Under A may be classed :—

(1) Want of sufficient knowledge or skill in flying, errors of judgment.
(2) Attempting hazardous feats or taking unwarranted risks in flying.
(3) Momentary loss of control, such as, for example, the accidental releasing of hand or foot controls, and not regaining them in sufficient time.
(4) Complete loss of control due to sudden seizure, loss of consciousness, etc.
(5) Loss of control owing to atmospheric disturbance, a violent gust of wind, for instance.

Under B may be classed :—

(1) Dangerous and inherent instability, due to faulty design. Failure of some essential portion of the aircraft owing to fault in design, insufficient strength having been provided to meet the stresses that the part or parts might be subjected to in the air.
(2) Failure of some essential part due to flaw or irregularity in the material of either engine or other part of the aircraft.
(3) Failure due to repairs or alterations not having been properly carried out, or to fittings, such as nuts, bolts, screws, etc., having worked loose

which had not been tightened up or otherwise made secure before the aircraft left the ground.

(4) Fire caused by leakage of pipe carrying petrol, or of tank, the petrol being lighted by spark or flame from the exhaust. This may occur in the air, but it is more likely to take place owing to breakage of pipes or tank when an aircraft strikes the ground.

The above comprise, so far as my experience goes, all the various causes to which the accidents to aircraft are primarily attributable, and it will be readily seen that many of them can be guarded against more or less effectually, if not entirely prevented, by suitable precautions being taken.

The Causes of Accidents Examined.

Taking the causes seriatim:—

A. (1) *Want of Sufficient Knowledge or Skill.* It is clear that this is not the most frequent cause of accident with fatal results, or more pupils and learners would have been killed than skilled pilots. Such is, however, not the case; indeed, it is surprising how few aviators have met with fatal accidents during such time as they were learning to fly, *i.e.*, during the period before they have obtained their pilot's certificates.

Errors of judgment have, however, caused a number of fatal accidents, through side slips, over-banking and diving too steeply when close to the ground, and over-estimating the speed of the aircraft.

(2) *Hazardous Flying.* There is little to be said in favour or in justification of taking unnecessary risks and indulging in what may be termed dangerous flying, especially if close to the ground, when a slight miscalculation or error of judgment may involve the pilot in a disaster.

It is hardly conceivable that an expert pilot should be so foolish as to jeopardise himself in such a manner.

(3) *Momentary Loss of Control* has, unfortunately, caused more than one fatal accident, and is one of those things that may happen to any pilot, however experienced he may be. Instances have occurred from time to time of control being temporarily lost in this way—luckily without fatal results—as, for example, in the Circuit of Britain last year.

In most designs of aircraft the elevator control is so arranged that if the pilot, when diving steeply, should slip forward in his seat on to the controls, the effect would be to make the aircraft dive more steeply still, and so render it still more difficult for the pilot to right his machine.

As a matter of principle, it would appear better to arrange the elevator control to work in the opposite manner, *i.e.*, to reduce the angle of descent instead of increasing it under the above circumstances. Unfortunately, it is late, perhaps too late, in the day to make any alteration in this direction, even if considered desirable, owing to the number of those who are accustomed to the present system.

(4) *Physical Failure of Pilot.* There has been one instance where loss of consciousness, owing to the pilot being in such a state of health that he was unfit to fly at all, caused his death, and in more than one other case this has been, with good reason, the suspected cause of the accident.

(5) *Violent Gusts or other Atmospheric Disturbances* have caused accidents which have terminated fatally owing to the pilot losing control, but generally only when the aircraft has been comparatively close to the ground and control could not be regained in time.

We now come to the causes which I have classified under the heading B :—

(1) *Faulty Design* has in the past undoubtedly caused unnecessary loss of life ; want of knowledge as to the magnitude of the forces which had to be resisted, the stresses to which various portions of the structure were subjected, and generally the conditions under which the aircraft flew, was responsible to a great extent in former days for the failures. Design and construction are now, however, thanks to the practical experience and scientific study of the problems involved, rapidly becoming exact sciences, and except in isolated instances of experimental machines, we may expect failures from bad design to become rare.

(2) *Failure of Material.* The sudden failure of some essential part due to a flaw or some other fault in material, whether it be of the engine or some structural part of the aircraft, is, I fear, an instance of the occasional fallibility of all sorts of mechanism and material and one that it is impossible to absolutely guard against. Accidents from this cause are, happily, of rare occurrence.

(3) *Faulty Repairs or Alterations Imperfectly Carried Out* have, unfortunately, caused loss of life on more than one occasion.

Lessons to be Drawn and Remedies Suggested.

Having now considered the various causes which originate accidents, and also in a general way their relative importance in the production of accidents, I will now turn to the lessons to be learnt from the accidents and their causes, and the means to be taken in order to avoid as far as possible their recurrence. I have purposely avoided referring to any names in connection with the subject of the causes of accidents, as my remarks are intended to be general, and no particular purpose would be served by indicating any particular instance, more especially since in nearly every case of accident some blame may be attributed, or thought to be attributable, to individuals, whether pilots or others. I should be extremely sorry that any statement I might make should be the cause of pain to anyone interested. For convenience I will again take the causes of accidents in the order in which I have scheduled them, and as far as possible suggest the natural remedies or precautions which should be taken to prevent them.

A. (1) Training of Pilots. I have already called attention to the fact that accidents of a fatal nature to pupils under tuition are rare. This in itself shows that the various schools where aviation is taught exercise great care in preventing their pupils incurring unnecessary risks, it being obviously to their own benefit and also to that of their pupils, actual and prospective, that accidents, from whatever cause, should be as few and far between as possible.

An error of judgment any pilot may be liable to; if it be as to height it may give rise to an accident more or less grave in landing, but if it be as to air-speed the effect may, in the case of an over-estimate, be far more serious. I am aware that some pilots consider that they can estimate their speed by ear almost as accurately as an instrument can tell them and for that reason do not consider an air-speed indicator as essential when flying; this may be true of a few, but it would appear that the majority would do better to trust to their instruments. There is no doubt room for improvement in the instruments themselves. They should be clearly legible in their indications, accurate, and so placed that the pilot has to distract his attention as little as possible from his work to read them; above all, however, they must be reliable.

(2) Hazardous Flying, etc. Common sense on the part of pilots themselves is the best safeguard against accidents from these causes.

No regulations, whether issued by the Royal Aero Club or even by the Government in the form of an Act of Parliament, will prevent a man doing foolish

acts ; if, as a result, he is killed, he has paid the extreme penalty of his foolishness. If he is injured only, he may be punished by law, but this will not disturb him so much, probably, as the knowledge that the accident was entirely his own fault and that he will not get any pity from others. Nor does he deserve any on that very account.

(3) Accidents occurring from momentary loss of control come under an entirely different category and would appear to call for improved designs, in which such possibilities as feet slipping off bars or pedals, or hands slipping from levers or wheels, or the pilot slipping bodily out of his seat, are guarded against. I have already referred to the possible effect of the latter.

The Safety Belt, pro and con.

One method of guarding against this is in everyday use with some pilots. I refer, of course, to the employment of a broad band or strap securing the pilot into his seat ; such a band or strap is and must be fitted with some reliable quick release device, whereby the pilot can instantly release himself with either hand when necessary. It is curious that whilst some pilots advocate very strongly some such method of securing themselves in their seat, other pilots are just as strongly against it. Probably there is a certain amount of sentiment about this ; men do not like being tied up or locked in under any conditions and in any place, even when they know that they can immediately release themselves. An impartial examination and consideration of the accidents which we have investigated leads me to the opinion that the advantages of the belt to secure the pilot in his seat more than counterbalance the disadvantages. In no less than six fatal accidents out of eighteen where the belt was not used, its employment might conceivably have saved the pilot's life, and might in more than one of these instances have enabled the pilot to regain control and avoid any accident occurring at all.

It is quite clear, I think, that pilots generally could not be compelled, at the present time, to strap themselves in to their seats ; neither could such a law be made until the great majority agree that it should be done owing to it having been clearly proved that the increased degree of safety thereby obtained justified compulsion. What can be done now is merely to keep records and compile statistics on the subject for the benefit of those concerned. I would only add that apart from the question of loss of control following upon losing their seat, pilots have been killed by being thrown forward violently against a strut or other portion of the structure on the aircraft striking the ground. This could hardly have occurred had they been secured to their seats ; on the other hand, it may be argued that a pilot may stand a better chance of escaping with his life if, not being strapped in, he jumps or is thrown clear of the aircraft on its striking the ground.

The Safety Helmet.

In any case there can be no valid objection to the use of a safety helmet, which, forming as it were a cushion or buffer between the head and the object struck, does, in effect, convert a blow, which would in most cases be a very violent one, into a push. If the same ingenuity that has been devoted to shock absorbers for landing skids, etc., were applied to contrivances, the simpler the better, for reducing the shock of impact on the human body in the case of a fall, by spreading it over a longer time, it is my opinion that more lives would be saved than at present is the case.

(4) The Physical Fitness of Pilots. Though sudden illness cannot, of course, be entirely prevented, still the instances when it occurrs will be very rare. I think everyone will agree that no pilot is justified in taking up an aircraft when he is in bad health or recovering from a serious illness, unless he has taken a

medical opinion on his condition, and this is to the effect that he is fit to fly. The matter is even more important if he contemplates taking a passenger or passengers up with him.

Medical Examination of Would-be Pilots Recommended.

I am strongly of opinion, which I know is shared by others, that no one who is not sound in body or limb should be permitted to learn to fly at all. Such a person who becomes a pilot is always a potential source of danger to himself and others. No real hardship could result if flying schools would only accept as pupils those who had passed a medical examination, with a view to establishing that physically they were fit for such work. The examination should be a special and thorough one, such as a candidate for the Army or Navy or Civil Service has to pass.

The question of the fitness of pilots who have already got their certificates is more difficult to deal with effectively. When pilots are employed in carrying passengers, as is now the case to a very limited extent, but may in the near future become widely spread, it seems to me that the public will insist on such precautions being taken in this direction as will ensure their safety being reasonably safe-guarded.

Atmospheric Disturbances.

The fifth cause of accident, viz., violent atmospheric disturbance overcoming the pilot's controls, is one which the human being cannot govern, and accidents from this cause cannot, therefore, be entirely prevented. It is quite clear, however, that they are rapidly being minimised by the use of more powerful engines and by the increasingly greater skill of the pilot, who, it would appear, is now able to right his aircraft from any conceivable position into which it may have been forced, provided he has sufficient vertical space in which to carry out the manœuvre.

I now come to the causes of accident enumerated under the second heading, B :—

The Proper Design of Aircraft.

The first of these, namely, the failure of some essential portion of the engine or aircraft owing to the design of such part having been wrongly calculated or calculated on insufficient data to ensure the necessary strength to resist the stresses to which it is put, is a fault that, a few years ago, might have been pardonable owing to the want of technical and scientifically accurate knowledge, but at the present time there is no more excuse for any aircraft being built without the proper calculations having been made to ensure a sufficient factor of safety according to the light of experience of all the parts under normal conditions of flying than there is for the construction of a bridge, ship, gun or any other structure being built in a similar manner.

It is common knowledge amongst engineers that in no mechanical structure can the factor of safety be infinitely great, and in such examples as a gun or aircraft it cannot be as large as in the case of a bridge or building where weight is not of prime importance.

No gun designer could be considered to blame if his gun failed when fired with twice its ordinary charge that it was designed for, as, for instance, occurred in the Boer War to a gun captured by the Boers and thus ill-used by them; nor could any aeroplane designer be held responsible if his machine failed under conditions of stress which it should never have been subjected to and was obviously never designed to resist. I would add that the proper value or figure of the factor

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of safety for various parts in a new science, such as we are dealing with, can only be arrived at by experience.

To build an aircraft by the so-called rule of thumb, and by that alone, is to court disaster, and it is a melancholy fact that valuable lives have been lost by the failure of such aircraft when flying. The responsibility for sufficient strength in the design is a grave one, and at the present time rests entirely with the designers and constructors of the aircraft, except in the case of aircraft supplied to the Government, in which case the approval of the design, if such is approved by the authorities, shifts a certain amount of the responsibility on to their shoulders.

If aircraft become common, as some people think they will be in the near future, it is quite possible, and, indeed, desirable, that their design and construction should be investigated and approved by some specially appointed authority before they are put into service.

(2) Choice and Testing of Material. As regards the second cause of accident viz., the failure of parts owing to flaw or irregularity in material, the only remedy, and even this is not an infallible one, is care in selection of suitable material for the purpose, and the taking of tests to ensure uniformity. Also, and this is an all-important matter in the case of the alloy steels in vogue to-day, the proper thermal treatment.

No failure of material, it must be remembered, occurs without some good reason, upon which careful investigation in most cases throws a light, and thus enables the particular cause of failure in the same part and under the same circumstances to be avoided.

Faulty Repairs.

The third cause of failure, viz., that due to repairs or alterations not having been properly carried out, or to want of adjustment or securing of parts after adjustment, is one for which the remedy is so obvious as hardly to call for remark or suggestion.

Here I should like to strike a note of warning.

Repairs should be only carried out under highly skilled supervision. To my mind it requires more skill and more technical knowledge to make a really sound job of a repair than to manufacture in the first instance. This is particularly the case where the repair involves a joint which has to be subjected to heat, it being impossible in many cases to subject the repair to a test or to assure oneself that the metal in the vicinity of the joint has not been weakened or altered in its physical properties.

The onus lies with the owners of the aircraft to see that no pilot is allowed to fly an aircraft unless it is certain, so far as can be ascertained by a careful examination made by a really competent man or men, that not only is the aircraft in a safe condition, but that the engine is working normally. If this had been insisted upon, at least two fatal accidents would not have occurred in the manner and when they did.

Precautions against Fire.

(4) As regards precautions against fire, either in the air or on the ground, the first step to be taken is the arrangement of the relative positions of petrol pipes and tank and ignition wires and exhaust exits or pipes, so that the petrol is kept as far away from danger of ignition as possible. The next is to provide

means for readily shutting off the petrol supply, in an emergency, close to, if not actually within the petrol tank itself. Fire originating in the carburettor through the engine firing back can probably be prevented by a suitable arrangement of metal gauze discs in the induction pipe, acting on the well-known principle which is used in the Davy Miners Safety Lamp. The agony of mind and body suffered by a pilot who, after a fall, is imprisoned in the aircraft owing to breakages or the position the aircraft has taken up on falling and is then enveloped in flames due to escaping petrol having become ignited, is too horrible to contemplate. Unfortunately, this form of accident has happened only too often, and it is difficult to see how it can be entirely averted. I know of no means at present.

In conclusion, I should like to call the attention of those more particularly interested in the design and construction of aircraft to the very excellent and complete series of recommendations made by the Departmental Committee on the Accidents to Monoplanes, 1912, and to be found in their report on p. 11. The recommendations in question are well worth careful study.

DISCUSSION.

Mr. MERVYN O'GORMAN, C.B.: I wish to make a few remarks in support of Colonel Holden as to the objects of the Accidents Committee, whose prime business is to secure the lessons which accidents afford and so obtain safety in flying.

This carries with it—

(*a*) The procuring of and sifting of evidence so as to get to the facts.

(*b*) The expression, when such is possible, of an opinion, which claims to be the best opinion the Committee can form with all the assistance it can command.

(*c*) The expression of recommendations based on that opinion.

Every one of these steps costs money. This Society is strong in talent rather than in money, its members are largely professional men whose resources are locked up in the success or failure of the solution they may have attempted of an extremely difficult problem—flight. It is not from them that further funds can possibly be obtained nor from the Aëronautical Society itself, the calls upon whose funds leave it at the end of the year with a minute bank balance of some £200.

It is a reproach beyond measure great that Britain fails to find the funds required for achieving the objects of this Society, whose members risk their persons or devote their time and skill unpaid to the cause. A capital sum of a few thousands, say £10,000 to be spent in about ten years, would maintain the trained staff necessary to make and check calculations, pay for tests and experiments, and put us in possession of our own skilled official who would be an independent expert to study and analyse these accidents—thus securing a better continuity of method and experience in inquiry than can be achieved by enlisting the good-natured assistance of some representative who may be available on each occasion to make a report on the spot.

With practice such a staff of engineers would become exceedingly skilled inquirers and when the hoped for lull in accidents occurs they will increasingly devote their time to the scientific and engineering side, for the common good.

The Accidents Committee does not commit the error of condemning flyers or constructors, but it does indicate weaknesses, errors and risks. Accordingly confidence in the fair play of the Committee is felt on all sides.

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The common sense of mankind admits that flying involves a measure of risk, and to find where that risk is greatest is not the same as imputing blame—to locate responsibility is not the same as censuring, and the Accidents Committee recognise this.

To find that a wire was responsible is not the same as blaming it since from our knowledge of physics we know that the wire did its best, and from our knowledge of decent men we know that so did its man who put it in.

It is splendid to realise the unquestioned impartiality and fairness ascribed the world over to the certificate of certain British bodies; thus, the R.A.C. technical trials are quoted and trusted everywhere, and already I find that French and American journals quote with complete confidence the dicta of the Accidents Committee.

The difficulty that exists in ascertaining the exact causes of accidents is immense, but I should like to add to Colonel Holden's list of causes, *wear and tear*, which may also be taken to include fatigue of metal, whether by the insidious vibration or the effects, unperceived at the time, of straightening a part which may have been bent in previous accidents. The decay from both these causes is often completely invisible to inspection. A bending may appear too trivial at the time for the re-straightening to be entered up in a record, and owing to the relatively short life of aeroplanes, neither wear nor tear have figured to any appreciable extent up to the present, or at least have not been noted upon.

One small criticism I would make—the lecturer remarks on the subject of hazardous flying that it is hardly conceivable that an expert flyer should be so foolish as to jeopardise himself in such a manner. Experience shows over and over again that it is precisely the flyer himself who, without any perceptible incentive, constantly persists in overlooking or neglecting the minimum of precautions, while those on the ground feel the burden of responsibility ten times more than he does. There is no disputing this—it is a fact. Very many flyers, if not the majority of flyers, do *not* adequately look round their machines before starting. Many do not insist on a new part, and *do* permit the readjustment, straightening, or patching up of an old part. The difficulty is temperamental. The flyer is keen to get on with his flying—he is often fiery and young. Prudence is not the quality that caused him to fly nor will it make him delay his flight till stresses have been calculated or factors of safety verified after a repair. He is, unfortunately, supported in this by those who pander to his temperament by scoffing at science and technical engineering work.

As regards the effect of gusts to which accidents have been imputed, I am inclined to think that in England, at least, on the occasions when flying accidents have been reported, we may have been mistaken in ascribing the prime cause of failure to gusts in the atmosphere, if by that we mean that these aeroplanes have been disturbed beyond the degree from which they could be rescued by the movement of the controlling organs. I do not say such could never occur, but I allude, tentatively, of course, to the cases which have occurred. I think that a moment's hesitation, lack of response, or fatigue on the part of the flyer after disturbance by a gust is the more likely primary cause of the accident. We must recognise fatigue and its effects on the man. The buffeting of the wind would increase such fatigue, and the design of an extremely comfortable, unfatiguing aeroplane is the remedy.

Very many motorists know that for no explainable reason an overwhelming somnolence has to be struggled with on occasions which arise quite irregularly on getting into the air. The purr of the engine accentuates this, and strange as it may seem to the onlooker or listener on the ground, the constant roar of the unsilenced aeroplane engine increases the fatigue and somnolence, though it may

not permit it to degenerate into sleep. From many years of motoring I can personally testify to this effect of the rush of air and the droning sound, the latter of which is supposed by some to produce a "hypnotic" fatigue.

To the remarks on the training of pilots I should like to add one golden rule—not only never fly without an air-speed indicator of some sort, but never intentionally exceed the maximum horizontal flying speed of the machine by more than 20%.

This is a counsel of perfection perhaps in the case of a flyer who has been immersed in a cloud and who, as occurs occasionally, may have completely lost count of his direction and whereabouts. Two reliable flyers have told me separately that they have found themselves flying towards mother earth when they emerged from the upper mists and thought they were proceeding on their way.

Now a flyer may, with some ten seconds of vertical diving (whether with the engine on or off matters not), acquire a velocity of 50% to 100% beyond what his maximum on the level should be. When travelling at such a pace, if he be compelled by proximity to the ground, or in error owing to the sudden appreciation of his risk flatten out his course with all the speed he can, he will impose on his machine stresses utterly in excess of those for which any known aeroplane has ever yet been designed.

Theory shows that to double one's flight speed increases the forces fourfold, and by turning the machine through its angle of maximum lift—which is what one does in flattening out—this force is increased four times again, thus giving a loading on the wings equal to the sixteen times the weight effect in horizontal fast flight. The full sixteen times stress could probably never be reached by a machine of high initial loading, whose normal attitude was a fairly great angle, say four or five degrees, but a figure of ten or twelve might be reached by a machine of light loading if the head resistance of its body work and landing gear, etc., were cut down by careful design, such as we find nowadays. Light loading has so many and such great advantages in speed range that it is one of the lines along which development is proceeding—and therefore the consideration of this risk must not be forgotten.

These remarks in connection with coming down out of clouds and diving suggest another most important safeguard, viz., keeping the aneroid in good order. On flying from one district to another, which may be immersed in fog, there is only the aneroid to tell the pilots that they are at a low enough level to beware of the ground, and of high objects.

Many a time has a flyer in a fog been awoken to keen attention by the sight of the top of a tall chimney, or spire, or poplar tree projecting through the general haze. Given that he has knowledge of the place he is approaching his aneroid should have told him beforehand that he was within 200 feet of the ground. If the aneroid needle had stuck and proclaimed 1,000 feet or more, the passing phantom of a chimney might not be believed to be such—for shadows and holes in the fog are common enough. Thereupon, if he made an attempt to look for the ground by a descent of a couple of hundred feet he would possibly strike it without being aware, at least not till the very last moment, and he would be in a critical situation.

It is early yet to advocate the use of two aneroids or other counterchecks on the aneroid, but that will come, just as we shall come to the compulsory use of two engines on aeroplanes, other than those for naval and military purposes, when the exigencies of the Service may warrant the taking of certain risks definitely for the purpose of obtaining certain specific results—and even then the future may well bring forth some middle course which ensures against the

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immediate compulsory landing due to engine failure. The whole question of what are the proper risks to be taken for the purpose of gaining military advantage is best left to the military authorities. Its consideration is entirely outside the scientific and engineering scope of this Society's work, and may in some measure depend on the standards current in foreign armies or navies.

In a totally different realm, it is just worth while to mention, for it is unquestionably overlooked now and then, the fact that when a loop is described in the air, aeroplane engines fitted with float feed carburettors are not in the same category as those with a hand-controlled jet. If centrifugal acceleration is kept continually greater than "g" the float feed still operates, though the effective head of petrol may be diminished. If it be equal to or less than "g," which is always foreseen by any flyer who uses the shoulder straps, the fuel is cut off and the closing of the upside down part of the loop under engine power is impossible.

The aspirations of flyers, who may be called partially expert, to describe a loop are growing in intensity very rapidly, and an indiscretion such as an unduly rapid dive with a view to closing well over the top of the loop (should the engine fail for lack of fuel) is a natural one, but it might also be fatal. The remarks of some successful exponents of this form of "outside edge" flying show that though they increase the speed by a preliminary descent it is by no means as great as might be supposed. The angle is not usually anywhere near the vertical nor is the amount of descent protracted. The exchange of information on these subjects would be matters of very great interest to all concerned in flight, and the application of a recording tautness meter to the wires of an aeroplane, when used in this way, would be so highly instructive that I should much like to be given the opportunity of obtaining readings if one of our skilful exponents would be willing to permit it.

Dr. A. B. LEAKEY, M.B., commented on the opinion expressed by the lecturer that aviators did not wear the safety belt because all men disliked to be locked up in or by anything. He questioned whether this was the real reason, and thought that the use or non-use of the belt was due to the experience of the individual's teacher.

He most thoroughly endorsed the lecturer's remarks as regards the examination of pupils for medical fitness. Nothing more lamentable could be imagined than a passenger-carrying pilot being medically unsafe. As regards the class of examination, he did not, however, consider that the Army, Navy, or Civil Service examination would be the kind, but rather a special one fitted more particularly to the peculiarities of aviation.

He would like to raise a point that he thought Colonel Holden had for the moment forgotten, and that was the precautions to be taken after an accident had occurred. He considered that at all Flying Schools there should be stationed one or more surgically qualified men, to render skilled aid to injured airmen. He scarcely considered that ordinary first aid was sufficient in connection with aviation accidents. Specially prepared men were required. One knew how when an accident occurred there was an immediate rush of mechanics and spectators to the scene of the accident, and how the pilot was dragged out from the wreck. While recognising the danger to the pilot of fire breaking out in the smash, thus making it desirable to remove him as soon as possible, surely all mechanics could be instructed that it is a very dangerous thing to move an injured man before first discovering what his injuries are. They could stand by and be prepared to act should fire break out, or other emergency occur, but otherwise wait until skilled assistance came. He had had in his own experience a case where the rough handling of an injured airman caused the loss of a limb, and another case where serious damage was done and a fatal result had only been avoided by good fortune.

He wished to comment on the totally inadequate padding often fitted round the edges of the bodies of aeroplanes. Often the portions which the pilot would be likely to strike first were not padded, or the padding was so slight as to be useless. He thought it was desirable to insist on plenty of good springy padding being interposed between all sharp edges or points and the pilot's body.

He thanked Colonel Holden for his extremely interesting lecture, and hoped that it would bear the good fruit which it merited.

Dr. A. P. THURSTON, D.Sc., asked why, when one considered the valuable information obtained from investigations into the causes of fatal accidents, an investigation into every accident in which the machine was damaged was not made, just as would be done in the case of a misadventure at sea. It had been his misfortune to witness several accidents, and he confessed to having learnt more from accidents that had been comparatively trivial in producing damage to pilot or machine, than from others where the pilot had been killed. He trusted ample funds would be forthcoming for the work of the Committee.

It appeared to him to be desirable that every pilot should be caused to undergo a medical examination of a specialised nature as regards certain senses. For instance, it was very important that every pilot should have the sense of balance and direction highly developed (the kinesthetic sense). We had all seen the ludicrous effect produced when a man had imbibed sufficient alcohol to affect his sense of balance. Disease or illness might impair or weaken the kinesthetic sense, which depended on the three semi-circular canals in the mechanism of the ear.

The control lever of an aeroplane was usually so arranged or connected with the controls, that if the pilot followed his instinct to keep in the line of resultant force and carry the control lever with him, the appropriate controls were operated to restore the machine to balance. He would, therefore, question the advisability of altering the controls, as suggested by the lecturer (in par. A (3)), as in that event it would be necessary for the pilot to act contrary to his instinct and nature in operating the control.

When one considered the accidents which had occurred under Class B (1), it seemed a lamentable thing that brave men should lose their lives owing to the faulty design of a structural part. If for any reason the machine were allowed to attain an excessive velocity, any attempted sudden "flattening out" would subject the structure to an excessive stress. The machine should be sufficiently strong to bear any load which would be placed upon it. If it were not possible to design the machine for an excessive speed then an automatic brake mechanism, such as he had described before this Society in 1909, could be fitted to check the speed when it exceeded a certain amount.

If the pilots who often took such liberties with their machines knew a little of the rudiments of structural design those liberties would never be taken. He remembered once designing a structure which, he was assured, would never be required to carry more than four people. He learned long afterwards that on a Bank Holiday as many as twenty-four people had managed to crowd in.

Although wind gusts and aerial disturbances did not appear to have been directly responsible for many fatal accidents it seemed highly desirable that further information should be obtained of the duration, nature, and magnitude of gusts. We now required information about the maxima and rates of change, as distinct from the average statistics heretofore obtained by meteorologists. He stated that he had designed instruments with this end in view, and hoped in the event of the information being interesting to publish results at a later date.

Mr. GRIFFITH BREWER: I was very pleased to hear Colonel Holden mention the value of air-speed indicators in flying. No doubt it is necessary for the pilot,

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even when he has such an instrument fitted, to judge the speed for himself, but an instrument which is not gravity controlled is always more accurate than the pilot's judgment, and I will give you an instance where the use of an air-speed indicator has been the means of saving the wreckage of a machine.

Two members of this Society, both energetic members, who are also members of the Accidents Committee, Mr. McClean and Mr. Ogilvie, are now flying up the Nile to Khartoum. They have been flying, or trying to get to Khartoum, for more than two months now, and this long time is not due to any lack of energy on their part. They have been delayed by engine trouble. They have an air-speed indicator on the machine, and Mr. Horace Short, who was one of the passengers, has given me details of an exciting adventure.

When flying up the Nile towards Assouan, the engine began to fail, and it became necessary to alight on the water. They threw out their anchor, which caught, and the stream being very fast swayed the seaplane roughly from right to left. For more than an hour they were like this, Ogilvie and Short all this time repairing the engine, and finally when they were ready, they started up the engine again, so as to enable McClean to taxi the machine above the anchor so that it could be got on board to continue their flight. Both Ogilvie and Short pulled at the anchor cord, and the stock of the anchor came away and they brought up the shank on the end of the cord. Fortunately the engine now worked well and they were able to get up into the air. Being, however, without an anchor, it was necessary when next they alighted on the water, to do so where they would not get wrecked by drifting on to rocks. The Nile at this part being of somewhat wild character, they had to fly many miles before they could come down without risk of damage. The engine again commenced to work indifferently, and if they had been dependent on their judgment for maintaining a safe angle of incidence, it would have been necessary to fly at a larger margin above the critical angle in order to prevent stalling. With the air-speed indicator in front of them, however, they could fly with the smallest margin of speed, and as soon as the speed dipped down too close to the known critical speed, they then put the elevator down so as to regain a safe margin. They flew like this for seventy or more miles, and Mr. Short told me that in spite of every care their altitude gradually decreased, and they could not get over the dam, but had to alight on the water on the north side. Had they not had this speed indicator, they would certainly have alighted on the water long before they got into a safe position, and the result would no doubt have been the wreckage of the machine on the rocks in the river.

I should like to add a few words on the subject of the factor of safety in construction. No doubt it is an excellent thing to have as large a factor of safety as possible, so as to ensure that the machine will not break, but one must not lay too much stress on those accidents which occur through structural collapse. It may be equally dangerous to add on weight in order to insure against structural collapse, if in doing so the margin of spare power is decreased owing to the overloading of the machine. We should therefore regard all accidents impartially, and not lay undue stress on accidents due to structural collapse as distinct from accidents due to insufficient control due to overloading.

Staff-Surgeon HARDY VESEY-WELLS, R.N., R.F.C. (*communicated*): I thoroughly agree with Colonel Holden's remarks as to a medical examination of all prospective air pilots.

In my opinion it is most important that before a person takes up flying he should be certified as being perfectly fit after a medical examination. Flying more than anything else requires perfect soundness of body. Few persons as yet realise the strains upon the heart and lungs in accommodating the system to the various degrees of pressure met with in flying, and I agree with Colonel

Holden when he says that in more than one case of accident there has been good reason to suspect physical failure of the pilot. I think that in any case of doubt as to the cause of the disaster the coroner should, on representation from the controlling authority, order a post mortem examination to ascertain if the pilot's condition could account for it; we could then learn something as to the suspected physical failure of the pilot. It has been argued that drivers of motor cars are not required to be medically examined, and they are just as liable to collapse and cause an accident; that argument is, I think, easily met; the conditions are so absolutely different. The loss of consciousness in a collapse probably does not come on so instantaneously that a motor car driver could not pull up, but it does come on too quickly to allow a pilot to land an aeroplane. Also it is the varying pressures, and possibly the strain involved in flying in bad weather conditions, that cause the collapse in a pilot not physically fit. High speed through the air and especially that behind the draught of a tractor propeller, probably causes some embarrassment in breathing. This will have a bad effect on an unsound chest. These high speed effects are not present in the ordinary motor car on the roads, although they may be present on a racing track.

Safety Helmets.

These, I agree, should be worn by every air pilot as they do give protection to the head in a smash and especially may save a fracture of the skull, from being thrown out on to the head, or from hitting a strut, or perhaps more important, in those cases where the head is thrown violently forward against a part of the machine. A well-padded helmet will minimise the striking force and so may save serious injury to the brain, apart from a fracture of the skull.

The helmet should not be too high in the crown, but just enough to allow of a certain amount of give without touching the head, and also should be well padded right round, as some types are, thus protecting the forehead and lateral region of the skull.

It has been argued against helmets—

(1) *They are uncomfortable.* This is not the case if they are a good fit. Any hat that is not a good fit is uncomfortable.

(2) That acting like the huntsman's top hat, if thrown on to the head, a helmet will cause the head to be bent one way or the other and so break the neck. This I think is extremely doubtful unless the crown is of exceptional height, and can be disregarded as an argument against the wearing of a helmet.

(3) That the helmet in a "tractor" machine, by its height and extra surface in the propeller draught forces the head back. There may be a certain amount of truth in this argument, and perhaps more so in some types of machines than others, but a good many pilots wear helmets and do not complain of this effect.

There is a growing tendency amongst aviators to disregard the protecting helmet and wear the skull cap with ear flaps, as worn by racing motorists. This, I think, is a bad principle, every pupil should be made to wear a helmet and he will probably continue to wear it during his flying career.

The Safety Belt.

These, in my opinion, should be worn by every pilot, and Colonel Holden gives most excellent reasons. The belt should be broad and have a simple and easily accessible release device. Experience seems to teach that a pilot on a "pusher" type should release the belt shortly before landing, so that in case of a bad landing and the machine being wrecked he can either jump clear or at any

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rate be thrown clear of the wreckage. In a case I have knowledge of, both pilot and passenger being unstrapped at the time of a smash, were both thrown out, the machine turned over and over and was completely wrecked. Had they been strapped in they must have both been killed, as it was they suffered severe but recoverable injuries.

In the tractor type the pilot must remain strapped in, and as in most tractor types he is well back, his position is usually aft of the crushed front, so the chance of being injured in the lower portion of the body or lower limbs is lessened. If he is unstrapped he is almost certain to sustain severe and possibly fatal head injuries on account of the head coming into violent contact with some portion of the aeroplane in front, or he will be thrown completely out and this with probably fatal results, unless he is extremely lucky.

Experience seems to teach, " Stick to the ' tractor,' but leave the ' pusher.' "

I do not think nearly enough attention is paid to the design of belts. An efficient belt must be of good breadth and the releasing device simple, two or more releases should be on every belt, so that a pilot strapped in after an accident may be sure of being able to get at one release. The releasing devices, if on the front of the belt, should be protected by some strong material so that a pilot doubled up may not find his releasing gear jammed or that he cannot get at it.

It seems to me that devices for not only releasing the pilot from his belt, but also to release the belt from its attachments should be fitted. Perhaps it could be arranged that a lever attached to the side of the nacelle would operate a releasing pin, freeing the belt from its attachment to the back of the seat or other attachment. A pilot wishing to release just before landing would not have to hunt about with his hands for his releasing gear.

A point brought out a good deal in accidents to " tractor " machines, which have dived to earth perhaps at no great speed, is that although the pilot is strapped in, the forward movement of the body is suddenly pulled up by a belt, or perhaps by the hand put out in front, but the head being on a flexible stand, the neck continues on its forward journey until the limit of neckbending is reached, when it is pulled up suddenly; this may cause a fracture of the neck, with serious results.

It does seem, in view of accident experience, that the restraining apparatus, instead of being a belt around the waist, should be a strapping around the shoulders with some attachment for restraining the forward motion of the head.

Colonel HOLDEN: In the first place I should like to express my thanks to those gentlemen who by so ably contributing to the discussion have much enhanced any value or interest that my paper may have had, and I should also like to thank my audience generally who have so patiently listened to what must necessarily, from the very nature of the paper, have been mainly an exposition of facts and data concerning accidents. If, however, it should happen that any of the suggestions made in the paper itself or during the discussion should be instrumental in preventing accident or loss of life in only one instance, I should feel that the paper had been useful and my efforts had been rewarded.

The CHAIRMAN: I will not detain you long, more especially as I am merely an onlooker, but a member of the Accidents Investigation Committee has a good vantage ground from which to observe, and in the first place, I should like to draw attention to the very able manner in which Colonel Holden has carried out his duties as Chairman of that Committee. As Colonel Holden has remarked, it is not at all an easy task to ascertain the real facts connected with aviation accidents, and I take this opportunity of referring to the extreme care and labour with which the chairman and members have conducted their inquiries, with the result, I believe, that the reports are thoroughly appreciated by all concerned.

Mr. O'Gorman made a strong point of the necessity for funds to assist in carrying out the investigations of the Committee, and there can be no doubt that for a moderate expenditure a great deal of very useful experimental work could be carried out in this direction. An objection is frequently made to public subscription of this sort that it is the duty of the Government to do the work and to a great extent that is so; this duty, of course, is recognised by the authorities and much experimental work is now carried out by the Royal Aircraft Factory, at the Flying Schools, and also by the staff of the National Physical Laboratory, but over and above the experiments made by the Public Departments there is a considerable amount of research which had perhaps best be left to private associations and committees.

Touching the question of the general nature of the accidents investigated, it seems to me that the majority have been due to the inexperience of pilots, using these words in no depreciatory manner but simply to express the fact that the art of flying is so new that sufficient experience to meet sudden emergencies must necessarily take considerable time to acquire. It would appear that there is an extraordinary difference between the capacity of various pilots and that we have scarcely yet arrived at any satisfactory system of selection or rather perhaps rejection.

The lecturer remarked upon the few accidents which have taken place at the schools of instruction. Is not this probably due to the selection of specially qualified and experienced men as instructors, to the great care they take, and to their staff of experts who maintain the machines in good working order?

There can be no doubt that most accidents occur at low altitudes and when alighting. It seems to me that we have yet to discover a more satisfactory system of landing than the present one; whether the addition of some form of air brake will give us the solution remains to be seen.

With regard to the remarks of Dr. Leakey on the possibility of medical examination of pilots, having attended a conference of three able medical gentlemen on the subject, I rather thought that they had agreed a special examination could be instituted which would serve the purpose required.

On the question of design, we have had several lectures in this Institution dealing with automatic stability, and I understood it was now generally agreed that a certain amount of inherent stability is very desirable as long as it does not interfere with effective control by the pilots under all conditions.

A very important matter is a well organised system of inspection both for detecting faults of the machines when first manufactured, and afterwards when in daily use, but such a system requires much time to perfect it and it is not reasonable to expect complete immunity from oversight in the course of a few months.

In conclusion, we must not forget the very remarkable improvement which has taken place in increased safety during the last three or four years. Quite recently I was reading some paragraphs in the "Globe" dealing with this side of the subject. They contained extracts from a French book called the "Martyrs of Aviation," by M. Roger Dépagniat; it appears from this work that whereas in 1909, one fatal accident occurred for 15,000 kiloms. flown, in 1912 one fatal accident occurred for 140,000 kiloms. flown. "Moreover this far higher degree of safety has been attained in the face of flights undreamed of a couple of years ago."

It now only remains for me to thank Colonel Holden, on your behalf, for his most interesting, useful, and admirably arranged lecture, and to say that we hope he will be good enough to give us a similar lecture each year, bringing the subject up to date. (Applause.)

A vote of thanks to the Chairman terminated the proceedings.

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THE TENTH MEETING of the FORTY-NINTH SESSION of the Aëronautical Society of Great Britain was held at the Royal United Service Institution, Whitehall, S.W., on Wednesday, 1st April, 1914, at 8.40 p.m. Lieutenant-Colonel F. H. Sykes, Officer Commanding Military Wing, Royal Flying Corps, presided.

Mr. HUCKS, before reading his paper, said that he wished to apologise for being late, which was explained by the difficulty of getting to the meeting. That day he had been flying at Lincoln, and had alighted at five o'clock. The only train he could catch went from Grantham, 28 miles away. It was a winding road and he only just did it by car. He hoped they would excuse any careworn appearance!

THREE YEARS' EXPERIENCE OF FLYING.

BY B. C. HUCKS AND C. GORDON BELL.

MR. HUCKS'S PAPER.

The primary object of the paper which I had the honour to be asked to read to you this evening, is not to be a mere recital of my experiences during the three years I have been actively piloting an aeroplane, although it has the title "Three Years' Flying Experience"; but I hope to be able to give to you a few points which should be of general interest, and perhaps beneficial to fellow-pilots and to others who have so kindly come here to-night to hear this paper.

Early Experiences.

I will first give you a few instances of my earliest failures, their causes and remedies. In fact, the solution of most of my troubles, when I look back, may be placed under the simple headings of inexperience and want of confidence, which, I suppose, are very nearly one and the same thing. The machines of three years ago, it is true, were not so efficient, nor so safe, but that fact has really, in my opinion, no more than quite a small bearing on the results of those days.

I will give you two instances of failure that happened to me soon after getting my brevet, purely through want of confidence, which I think I can safely say, with my present experience, would not occur again, even supposing I were piloting the same machine, under identical conditions.

The first instance occurred in attempting a cross-country flight from Filey to Leeds, on a 50 h.p. Gnome Blackburn. All went well for a distance of 18 miles, when suddenly one cylinder ceased firing, a state of affairs which, to-day, would not be very disconcerting, but then, purely through want of confidence, or due to inexperience, I was seized with—shall I say, "cold feet"! I had visions of the machine dropping like a stone, if I dared to try to fly it another yard, so all that was left was to make a wild plunge into the very nearest field; never mind if it was suitable or not as a landing ground—get down somehow, as long as you get to earth. Yes, those were really my feelings in those days, and, I may add, those of most pilots—and the result was, that the field being unsuitable (full of cattle) and rather small, I dived to make the best of it and smashed the machine to pieces! Now, exactly the same thing happened to me a little later on, under practically similar conditions. Very likely a good number of you present to-night will remember the start, on the Monday morning from Hendon, for the circuit of Britain in 1910. What a perfect flying morning! I was flying the same machine that had been smashed up in Yorkshire, and exactly the same

trouble overcame the engine after 35 miles; one cylinder began to miss-fire; the same terror seized me, and I chose the nearest field, likewise wholly unsuitable, having a practically invisible barbed wire fence across the middle. It was not that I made a bad landing, although the machine was wrecked—the field was hopelessly impossible, and the irony of it all was that less than 400 yards away was a veritable aerodrome waiting for me to land in, had I only had the confidence to try to fly the machine on the six cylinders which were still firing.

The reason I want to bring these two instances to your notice is that you will see what a simple thing it took in the earlier days to bring about disaster.

As I have said before, I would willingly risk flying the same machine to-day, under the same conditions, and there is very little doubt that I should succeed in keeping it in the air, even with its loss of power, until I had found a suitable landing-place.

I daresay there are numbers flying to-day who could relate instances where, had they but known then what they know now, things would have turned out very differently.

The Improved Skill of present-day Pilots.

I consider that the present-day high standard of flying is due, far more to the improvement in piloting than to the improvement in machines. One has only to be at Hendon, any very bad windy Saturday or Sunday, to see that. It is true that most of the machines there are of the later types, but then see the wonderful way the old Farman-type biplanes are flown. I consider those machines are only slight improvements on the machines of three years ago, and yet they are put through evolutions which, at that time, were not even dreamed of. I can take a good example of the way improvement in piloting has out-distanced improvement in machines—in the case of myself, my "looping" Blériot. Most of you know that there is very little difference between that machine and the 50 h.p. Blériot of three years ago. The difference, at any rate, is so slight that any 50 h.p. Blériot of that period could have been altered in a very short time so as to make it safe to be flown much in the same way that my "looper" is flown. I can remember Mr. Grahame-White, at the Boston meeting in U.S.A., in the autumn of 1910. He had, as far as I know, the first 50 h.p. Blériot that ever went into America.

I can imagine myself with my present knowledge and experience just altering his machine slightly, and starting off on a flight at that meeting, and causing some slight sensation by turning the monoplane upside-down, and putting in a few loops—and it could easily have been done. All that was wanted in those days was just the confidence and knowledge that the experienced pilot of to-day possesses.

I have been telling you what a really valuable asset confidence is to a pilot, and how essential; but there's such a thing as too much confidence. I remember an incident which was probably one of the narrowest escapes I've ever had through over-confidence. I was acting as chief pilot for the Grahame-White Aviation Company, at Hendon, and had confined most of my flying to mono-planes (Blériots) when it occurred to me that it would be useful if I likewise mastered the bi-plane, and accordingly I very comfortably started off on an old "school" Henry Farman type machine, and I found things quite all right. The machine seemed just a little strange, rather more cumbersome and slower to act than the machine I had been used to, but I felt thoroughly at home; in fact, rather too much so, for after about two or three circuits, I decided to try a few "stunts," the first in the form of a vol-plane from about 300 ft. Now, firstly, before proceeding with the relation of this incident it is necessary to explain certain characteristics of the two machines in question.

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The monoplane being a tractor machine, the propeller slip-stream gave slightly extra support to the main planes, the result being, when the engine was switched off, that this slight extra support was withdrawn, and the machine assumed a more or less natural diving angle, without requiring first to have its nose put down. In the case of the biplane, the machine, being of the pusher type (*i.e.*, with the propeller behind the main planes), has no lift on the main planes through the propeller slip-stream, but the reverse happens—the tail planes alone receive an extra lift through the propeller draught, and, when the engine is stopped, this extra lift promptly disappears from the tail, so that the tail drops, and the machine tends to assume a climbing position as soon as the power is cut off.

This is what happened in my case; instead of putting the nose of the biplane down into a gliding position, I switched off, expecting much the same to happen as with the monoplane, *i.e.*, that it would naturally take its own gliding angle. But, as I stated before, I was over-confident, for the tail dropped on switching off, and the machine tended to climb, in spite of the fact that I had the elevator pointed hard over for descent. The engine refused to pick up again, owing to over rich mixture, the machine came to a standstill in the air, then commenced one huge side-slip to within, as eye-witnesses state, 25 ft. of the ground, when it pulled itself up, starting another side-slip in the other direction, like the swing of a pendulum, eventually landing sideways on a side-slip, but fortunately sufficiently flat merely to carry away the whole of the landing chassis. There is not the slightest doubt that sheer luck and no judgment saved me. I consider that the natural tendency possessed by most tractor machines to assume a nose-diving position, if left to get out of awkward positions, is an enormous advantage over the pusher type machine, which I fear does not possess that ready tendency. I have mentioned this instance to illustrate the dangers of over-confidence.

Flying in Bad Weather.

I will next deal with the subject of flying in bad weather. I think all pilots here to-night will agree with me that the worst enemy to flying is not wind or rain, but fog. I know which I would rather fly in. I think perhaps I might illustrate an instance of the extreme unpleasantness—not to say danger—of suddenly running into fog or mist. In May, 1912, I was testing-out, at Issy-les-Moulineaux, near Paris, a 70 h.p. Tandem Blériot, giving several of my friends "joy rides" from the Issy ground, round the top story of the Eiffel Tower, and back. I think I had my third passenger with me, and was just about to make the turn round the top of the tower, when the machine was suddenly enveloped in a low-lying cloud scudding across the sky. I could see nothing—barely my wing-tips—and, knowing my close proximity to the tower, such a thing as colliding with it was not an impossibility. However, I swung the machine at right angles to its direction by the compass, and when I eventually came out of the cloud I had several hundred yards to spare from trouble.

Fog.

A bad fog makes cross-country flying very dangerous and almost impossible. Many narrow escapes are on record of pilots flying over steadily rising ground, and assuming by their barographs that they were at a safe height, but only to find that, on suddenly coming out of the fog, they were literally skimming tree tops. The only redeeming feature—if one can call it so—is that high winds and fog do not go together.

Rain.

Rain is distinctly unpleasant, and blurs the landscape. Heavy rain becomes, at times, almost unbearable, owing to the force with which it is driven against the face, principally by the speed of the machine, and also by the propeller slip-

stream. To give one an idea of what high speed in very heavy rain will do, the following instance will suffice. Mr. Sydney Pickles was flying my 70 h.p. Blériot from Coventry to Birmingham last year. On the journey he encountered a heavy rain storm. On landing, he complained of the pain he had suffered, and upon examination of the propeller it was found that the leading edge of the blades had the appearance of having been gnawed by rats, such was the force of the propeller meeting the rain. I remember seeing Mr. Grahame-White make quite a lengthy flight at Boston, U.S.A., in a fairly heavy rain. His face was much swollen on landing owing to the bruising effect of the rain. I myself had quite an unpleasant experience in only a fine drizzle—sometimes called a "Scotch mist." I was persuaded to try a new type of goggles, made of a non-inflammable celluloid, but these had the disadvantage (which I had to find out in practice) of badly steaming up and practically obscuring the view entirely, in a fine rain—in fact there was nothing left but to remove the goggles altogether, the result being that the whole force of the rain, fine as it was, came into the unprotected eyes. The pain was so intense, although it probably only lasted half a minute (time in which to effect a landing) that I suffered with very bad inflammation of the eyes for two days afterwards. I do not think rain affects the flying capacity of the machine to any great extent. Continued rain would, I believe, show its effects on the propeller first of all by being driven into the joints, causing the parting of the laminations.

Winds.

In the case of trouble owing to wind, the velocity of the wind can practically be neglected, excepting that a very strong head wind makes a cross-country journey a bit longer than it might be. When I say that the velocity of the wind can be neglected, I mean that the nature of the wind is of far more consequence to the pilot of to-day than its speed. I have flown, whilst at Southport, in 1912, in a wind which I believe I am perfectly safe in estimating at 45-50 miles per hour, because of the little headway the 65 m.p.h. machine made against it, and yet the wind gave no more trouble than a flat calm, because, provided one landed into it, the machine did not run a length and a half; it made landing easier, the machine climbed quicker, and the air seemed more buoyant. The wind in this instance was blowing directly from across the Irish Channel. During my earlier experiments in flying at Filey, on the Yorkshire coast, I invariably found that the wind, no matter what its velocity when blowing off the sea, was perfectly steady, and gave no trouble whatever to the pilot. I think that perhaps one of the most noteworthy examples of the strange behaviour of winds, was the start from Brooklands of the first "Daily Mail" Circuit of Britain. The day was a sweltering hot one, with not a breath of wind, and yet I suppose it surprised most of the pilots, and not a few found it too bad altogether. I myself, then quite inexperienced in flying in bad winds, after a start found the conditions so bad that I deemed it wiser to return and wait till conditions improved, and all this happened on a sweltering hot summer afternoon, with little or no wind.

The cause of such unexpected conditions was due, I think, to the intense heat, which made the atmosphere literally boil, causing eddies, upward and downward currents, and general disturbances of the air.

There are several outside causes which affect the nature of the wind. One of the chief is the contour of the surrounding country. I mentioned previously how, when the wind blew from across any large expanse of water, it was invariably steady and gave little trouble to the pilot. I have found that when the wind is blowing from across hilly, undulating country, it is a more difficult wind to deal with, owing to the manner in which the currents are broken up and deflected.

In the summer of 1912 I was flying at Strathpeffer in the north of Scotland. My flying ground was at the foot of Ben Wyvis, a mountain 3,500 feet high—in fact, the whole surrounding country was mountainous. On this day in question

there was very little wind (ten or fifteen miles per hour), but I found the way in which the currents were broken up by the mountains was most marked. One usually finds that after reaching a considerable altitude—say 3,000 to 4,000 feet—the wind gets steadier, no matter how gusty it may have been near the ground. I remember that during one flight at Strathpeffer, I reached a height of 5,800 ft., and even at this height one found the unsteadiness of the wind most marked, an effect due in all probability to the proximity of the mountains.

I certainly shall never forget the view I had on that day. From a height of 5,000 feet, I suppose I could see some thirty miles away. On one side was the sea—the Moray Firth—on the other, as far as the eyes could reach, were mountains standing up like saw teeth, with large glistening lakes here and there. The prospects of an enforced landing in such a place were certainly not over pleasant, and it was not surprising that after the wind had traversed such country, it became a bit troublesome to airmen.

There is another instance in my memory which seems to show that the wind is affected by causes other than contour of the land. In the spring of 1912, whilst flying at Weymouth, I found that whenever the machine seemed to pass from the land to the sea, it received a somewhat severe shock, just at the moment of passing from the land to the water. This would happen from a height of about 500 feet. At first I took it to be a coincidence, of just a gust striking us at that moment, but it seemed to happen so repeatedly that I believe the change from the land to over the sea was the cause of it. I cannot assign any definite reason for this, as there were no cliffs, and the land ran down gradually to the sea. As it was quite a warm sunny day, the sudden change of temperature from land to sea probably set up vertical currents.

"Air-Pockets" a Misnomer.

I have so often been asked the question whether I ever come across air-pockets when flying, and what happens when I do. Personally, I think the term "air-pocket" is a misnomer, as most people are thereby led to believe that such a state of affairs exists in the atmosphere as a partial or semi-vacuum, which, as it were, goes floating about, dealing out destruction to the unwary airman. I really believe that there are quite a lot of people who have not troubled to think out the matter, and actually think that, owing to this partial vacuum, the aeroplane suddenly drops, as its support has gone. The so-called "air-pocket" is, in my opinion, merely a quick downward current. A machine will at times seem to drop as if its support were suddenly withdrawn, behaving much in the manner one would expect it to, if it were possible for it suddenly to come upon a partial vacuum in the atmosphere. But I believe the machine will do, and readily does, exactly the same thing in the other direction, *i.e.*, it gets suddenly caught in an upward current and gets shot up rapidly, the only difference being that the downward current is more noticeable, because the human system is more sensitive to sudden descents than to ascents. I think most of you will agree that, when travelling in the vertical plane in a fast elevator, or lift, one feels the rapid descent far more than the ascent, although the speeds in either direction may be the same. Thence, the pilot will notice his machine drop more readily than he feels it suddenly ascend. The term "air-pocket" is therefore, undoubtedly, a misnomer.

The Efficiency of Pilots.

There is one point which I should like to bring forward here, *i.e.*, the case of a pilot who undertakes the flying of all and sundry types of machines. Whilst it is a very valuable accomplishment, I can't help thinking of the old saying, "Jack of all trades—master of none." I do not suggest that an airman who pilots equally well some dozen different types of aircraft, is not master of one of them, but I do think that for the utmost efficiency a pilot should specialise on one particular make of machine. I suppose that for "Service" work it is most

essential that pilots should be able to fly any machine which they may be called upon to pilot in an emergency.

But every type of aeroplane has its peculiarities, and it is quite conceivable that a pilot may get confused with these differences. On the other hand, in my opinion, if a universal standard control were adopted, it would tend to reduce this trouble. One can imagine the confusion there would be, supposing a certain make of motor car were steered by the feet, and one were suddenly called upon to drive another make that is steered by the standard wheel steering. I mention this as analogous to the difference in the controls of some makes of aeroplanes.

After a pilot has gained a little experience, he does all the controlling of his machine absolutely *instinctively*. He never has to think what to do; the whole operation comes to him as naturally as walking. When walking the deck of a ship which is rolling and pitching badly, it is not necessary to have to think out your movements in order to effect a safe crossing of the deck. Everything is done by mechanical instinct, exactly in the same way that a pilot will fly his machine on a rough windy day. It is easy, therefore, to see how confusing a machine would be to a pilot who is not used to that particular type of control.

Efficiency of Machines.

High speed has been sought after by designers in many cases, at the sacrifice of other desirable qualities in a monoplane. I think that a machine which has merely very high speed as its feature is of very little practical use. An aeroplane should, I consider, be judged from the standpoint of all-round efficiency. An aeroplane that will fly at 100 miles per hour, but requires about 400 yards to land in, is too dangerous a proposition for practical use, as one would have little chance, in the event of engine failure, of effecting a safe landing on a cross-country flight.

Equally dangerous would be a machine that would not glide sufficiently well with the power cut off. For instance, there are some types which, owing to rather more head resistance of, say, the landing-carriage, have a slightly reduced speed. Well, suppose that that landing-carriage, with its extra head resistance, is capable of withstanding far greater shocks than the chassis that gives a minimum head resistance, surely it is worth while sacrificing—say three or four miles per hour, if by so doing you can land ninety-nine times out of a hundred without trouble, as against—say nine times out of ten with your faster machine.

I mention these points, as so many people are apt to judge the qualities of an aircraft entirely by its speed, and not so much as should be, by all round efficiency.

Looping.

I now come to the last part of my paper, which is concerned with "looping-the-loop" and upside-down flying. My advice to any pilot who contemplates these experiments—and they are well worth considering, as I will show later—is, primarily, "be certain that the machine which you intend to pilot through these extraordinary evolutions is suitable for the work." There should be no question as to its strength. The bracing of the wings, especially if it be a monoplane, should be such that it will stand as big a strain on the top side as on the under side of the wings. The range of control should be greater than is necessary for ordinary flying, so that the machine will have no hesitation in responding to the sudden calls on the control. The machine must be so balanced that it will assume a nose dive if the controls are abandoned at any considerable height.

The whole secret of the safety of this freak flying lies in the fact of allowing sufficient altitude in which to recover from any precarious position the machine may assume. I have found out that there is no position that the machine can be put into from which it cannot easily be brought back to the normal again,

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provided, as I say, that there is sufficient altitude allowed in which to effect the recovery. This one point alone, when clearly grasped, adds enormously to that most valuable of qualities which a pilot can possess, confidence.

There is nothing extraordinary required in the piloting of the machine. I find it does practically everything you set it to do in the way of tumbling about in the air. One golden rule, which I had thoroughly drilled into me by M. Louis Blériot, when practising for the looping at Buc, was to use the controls "doucement," *i.e.*, gently, as one can easily see that a sudden jerk on, say, the elevator, would throw an enormous strain on the *fuselage*, whereas if that movement were made "doucement," the strain would be negligible. Much the same thing applies to the driving of a motor car—one driver can steer a car fast round a corner by handling his steering with a sensitive touch, whilst another with harsh hands will cause the car to skid, at exactly the same speed, which shows how a much greater strain is thrown on the machine merely by the touch on the control.

The machine on which I have at present made several hundred loops, has a far wider range of control than a standard machine. The wing warping is nearly four times as effective, and the tail elevator is considerably larger. The result is that the machine readily answers to the control. For instance, when doing a perpendicular nose dive, the machine responds immediately to the elevator and flattens out without the slightest hesitation, a point of the greatest importance. The same thing happens with the warping. The machine can be banked up so that the wings are perpendicular, by simply turning rather sharply, and warping at the same time.

The machine is fairly easily got into the inverted position, but it is very noticeable how unstable it is when in that position. It requires extreme care in balancing it, as it were, because the moment it is allowed to get only slightly out of the level, it will side-slip and turn the right way up. This, after all, is only what would be expected, as it shows its natural stability, and its tendency always to assume its normal position.

The machine cannot be made to climb when inverted, although the engine may be turning full speed. There is, in fact, very little support in the wings when the wrong way up. If one tries to keep even the same level, with engine full on, the machine will stall, and side-slip round to its normal position again.

The only way I find my machine can be kept in an inverted position for any period (my longest timed inverted flight so far is 2 minutes 5 seconds, and even that seems quite a long time to be hanging head downwards) is by allowing it to plane downwards the whole time. The control when hanging head downwards is not so confusing as might be expected, because the pilot still bears the same relative position to his machine.

"Strapping in"—The Safety Belt.

Needless to say, one has to be very securely strapped in for these evolutions, as otherwise one would certainly fall out.

I find it a great advantage to be strapped very solidly and tightly to the seat. I have straps round the waist, thighs, and shoulders. It is true I feel much as if I were in a strait-jacket, but I consider that far better than if the straps were at all slack. When you are tightly strapped in, you feel absolutely solid with the machine, and a sudden bump or jerk does not dislodge you in the slightest.

Most of you who are present have no doubt travelled some time or other in the back seat of a fairly fast car, and you must know how unpleasant it is when that car goes over a small bridge or culvert on a country road, and you leave the seat and shoot up about a foot. Well, there are times when an aeroplane treats one much in the same way, and you will then see the advantage of being really tightly and rigidly strapped to your machine.

Of course, there is a serious disadvantage to being strapped in rigidly to the machine, and that is the difficulty of getting out quickly in the event of even a minor mishap. What is wanted is some certain quick-release device, which cannot by any chance come undone until required. I consider that given this device, strapping in, and rigid strapping in, would be adopted generally.

The Value of "Freak" Flying.

I now come to the question of the value of this "freak" flying. There is not the slightest doubt that every pilot who has looped-the-loop, or flown upside down, has benefited considerably by so doing. His confidence has increased enormously, and even those pilots who have only seen others do it must benefit. Surely it is nice to know that a machine can be wantonly turned over and righted again. I can remember, not so very long ago, before the looping era, that a very bad gust of wind struck my machine, which sent the planes up vertically. I thought something very serious was going to happen, but the machine recovered somewhat quicker than I did myself. There is no doubt that the same thing occurring to-day would not be nearly so disconcerting, because one knows that it is possible to recover (if there is altitude enough) from any conceivable position the machine may be thrown into.

Then again, it will bring about a general strengthening of machines, because a machine which has successfully looped a number of times has surely proved itself strong enough to withstand the strains of ordinary flying.

It is true that one does not fly the wrong way up because there is any advantage in so doing. The critics have said that most men are quite satisfied in being able to fly the right way up. But the same thing can be applied to most things. One might as justly say, "What purpose is served by driving a motor car at the rate of two miles a minute on Brooklands track? Most men are content if they can drive their cars at half that speed." But, in each case, they are extreme tests, which record the progress of science.

Mr. C. GORDON BELL apologised for not having prepared a paper, but said he would do his best to make some remarks of interest. He gave an amusing account of his early experiences, starting in October, 1910. He then brought forward some points on which he asked for information. Why was it that certain clouds, when he went near them, made his compass want to dance the tango? He then proceeded:—

I now come to two important points that I think deserve attention. These are keel surface and air-brakes. If we look at the diagram that I will now draw on the board of this little tractor biplane, we shall see that there is a large area of keel surface. Now in order that the machine should be stable this keel surface must be properly disposed about the centre of gravity of the machine. Mons. Eiffel has pointed out that the centre of pressure of a flat surface inclined to an air-stream lies between two-fifths and one-third from the leading edge, whereas if the section of the plane was a streamline shape the centre of pressure is considerably more forward. Now let us suppose our little tractor biplane starts to sideslip. This will mean that the wind will strike it in the direction shown by the arrow, that is the streamlined body will be at an angle to the relative wind. Its centre of pressure, therefore, will be up towards the bow of the machine, while its centre of area will be considerably further back. The behaviour of the machine is therefore likely to be different than the designer intended, owing to the fact that there is not sufficient keel surface behind. In a tractor machine this effect is accentuated when sideslipping, owing to the fin effect of the propeller, which is quite considerable.

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Then I want to mention the question of having a vertical fin above the centre of gravity. If one is fitted and the machine starts sideslipping outwards, the effect of this top fin is to bank the machine up and prevent sideslipping. I should like to know of any experiments that have been carried out in regard to top fins.

Now for the other point, air-brakes. We all know that machines can now be made with a speed range of from 90 miles an hour to 35 miles an hour, but does not the latter take some getting. When you want to land you can easily use up two or three fields in simply chasing your slow speed. We have got to find some means of spoiling the gliding angle.*

When we come to fit air-brakes, however, we find that they have disadvantages of their own. To be efficient they must not tend to produce any upsetting effect on the machine nor mask the controls in any way, and they must be capable of being put on and off whenever required. I don't know what solution will be found for this problem, but the split rudder that Mons. Bleriot has been experimenting with seems quite promising.

DISCUSSION.

Mr. LEONARD BAIRSTOW said that he had been much interested in the points brought forward by the two lecturers. As regards the stability questions that had been raised, they could be, and were, the subject of calculation. Experiments had been carried out at the N.P.L. into the question of keel surface, and the data discovered for inherent stability would, he thought, be of use to constructors. One could easily make the rudder area too large, and if the machine started side-slipping inwards a spiral nose dive would develop. On the other hand, if the rudder were too small the machine would be directionally unstable and might even turn tail foremost. A top fin was all right, but had disadvantages, and its equivalent, that is, a dihedral angle, is much better.

Mr. MERVYN O'GORMAN: I am inclined to wonder whether Mr. Hucks ascribes to increase of confidence too large a share in the credit for the admirable recent progress of pilots like our two lecturers. His increase of skill, which includes sensitiveness to detect and check the beginnings of an undesirable movement, the sure eye for a good piece of ground, practised observation leading to the anticipation of eddies over trees, hills, hot patches and cold patches; all these things make more for his safety, perhaps, than the growth of his confidence.

I am not sure that he exercised the virtue of confidence to a greater degree the first time that he quitted the earth on a machine which he had not yet the experience to criticise or appraise—or the first time he made an inverted flight—than he did throughout all the flights in which he succeeded so well, and which he ascribes to the development of confidence.

Mr. Hucks's remarks seem to convey rather than to state that flying into a head wind assists the climbing speed of the machine. In a spirit of inquiry I should like to know if he can elaborate the point. I am not aware of any reason why the aeroplane should climb more rapidly against than with the wind, though it may be so. I do, however, see that against the wind a machine may leave the ground in a few yards and show upward movement while the longitudinal progress over the earth is slow, so that the illusion of climbing quicker would be very strong indeed.

Instrument readings would help better than impressions, therefore, but even individual cases supported by the barograph, unless they give consistently better climbing against than with the wind, would not prove the case. The occurrence

* See remarks by the late Lieutenant Parke, page 247, "Aëronautical Journal," October, 1912.

of remarkable climbing rates, by reason of the air movements, has been recorded on a flight of Mr. Spratt's. On a machine which could not, by the known available margin of engine power, climb more than 480 feet per minute, he achieved some 900 feet per minute, as shown by the recording barograph, when not flying against the wind and when there was no extremely high wind blowing. In this case he was evidently in the air upcast, such as is usually topped by a cumulus cloud.

It is, perhaps, more true to say of the older biplanes of propeller type (the type called " F " type in the Army) than of the latest machines that these did not, when the engine was cut off, adopt their gliding angle of descent. At any rate, the slip stream of the propeller over the tail need not, indeed, I think one may say nowadays should not, be such, and the balance so adjusted that, when interrupted, the aeroplane adopts the " cabré " attitude.

Such a machine can, we now know, be made so that when gliding downwards, if the engine be opened out, it will, *without any elevator movement*, the elevator being supposed straight and kept so, first increase its speed and soon turn its nose up, and if the throttle be kept open, climb. The day is not far off when all aeroplanes will be expected to do this.

The effect of hills or mountains, though doubtless usually causing most irregular eddies in the wind near them, do not always do so. I remember, when at Vienna-Neustadt, being informed by the very courteous Colonel in command of the Austrian Flying Corps that at all ordinary levels no particular irregularity was caused at the magnificent aerodrome there by the wind over the high mountains bordering all one side of the ground. That is indeed an ideal aerodrome, where a level grassy stretch some eight miles long by four miles wide, quite flat, offers every facility to the pupil for learning his art before he ventures into bumpy air and over bumpy ground—which certainly both exist in Austro-Hungary and elsewhere.

In fog there is little for the flyer to go by other than his aneroid, and this, which is one of his lifebuoys, must on no account stick so as to indicate a high level when he is, in fact, low down. Even when it is right, however, he cannot alight by the information it gives him, and fog remains an immense difficulty.

As regards the specialising of the study of particular machines by particular flyers, it is obviously a good point for Service purposes that the unit consisting of man and machine should be so wedded that the combination is as efficient as possible, and made efficient as rapidly as possible. This is got by specialising. Such a flyer is an admirable critic of every small experiment or variation made on that particular type. For this gain, however, he pays by being less able to weigh the advantages of one type against another. Men like our two versatile lecturers, who between them cover the knowledge of as many machines as one could ever expect two men to know, are in another category, and for research and experiments which must cover all sorts and kinds of aeroplanes, large and small, with various organs of control, of balance, and varying degrees of stability, there is still—indeed, there always will be I think—an important place for the versatile flyer.

Mr. Bell's record of the effect of clouds in making the compass "dance the tango " is very astonishing, because difficult to understand, and difficult to get in such a definite manner as to be explained.

That the clouds actually caused any visible variation of the compass by the electrostatic or electromagnetic effects seems improbable, unless, perchance, the aeroplane itself were discharging very large quantities of electricity, which, at the potential difference needed for disruptive discharges to a body of vapour, means enormous sparks. Electrostatic attractions on the needle are possible, but seem unlikely to cause deflections. What appears far more probable is that the aeroplane in question had a two-bladed airscrew; that the flyer was not

aware of the fact that he was turning ; that such turning with a two-bladed screw caused the well-known gyrostatic vibration, and that this vibration chanced to be of such a period and direction that the needle was caused to make big deflections.

Of this deflection by suitable vibration there is ample proof, and the experiment is easily performed by anyone who has a compass. The best modern compass made shows it quite clearly.

It is very good hearing that a flyer of Mr. Bell's experience should draw attention to the value of fin distribution. For air purposes the top fin which he advocates has much to be said for it. If it is proportioned, and the rudder and other fin are proportioned in the manner indicated in Mr. Bairstow's recent lecture, nothing but good results will be obtained from it in the air. But the problem of safe flight is nowadays the problem of safe landing in a wind, and from this point of view we find a totally different outlook. What is wanted is a righting moment, not a side force, even when we are in the air. The top fin gives a righting moment, as Mr. Bell says, but it also unfortunately introduces a very serious side force with side winds. This side force at a high level in a machine is most inconvenient when by reason of the configuration of the ground the aeroplane must be brought down across, or partly across the wind. Very many experiments have been made on this at the Royal Aircraft Factory with excellent results in the air. To get over the side force on the ground, the fin has been mounted on a pivot at its leading edge and "cast off" so as no longer to be a fin when alighting. In this way it has been possible to land across a wind of 25 miles per hour, but the complication is unnecessary if an adequate dihedral angle be used and the device was abandoned. It was pointed out in a lecture on Stability Devices before this Society that the balanced warping wing machine wiped out all "dihedral angle" effect, since the very design provided that any difference of pressure on the two wings should be balanced out. This forms quite an important argument in favour of wings with hinged trailing edges or flaps, since with the fixed part of the wings the full dihedral angle effect can be retained, and by this means the restoring couple which a fin affords is secured, without exposing oneself to the side forces which a fixed fin entails.

Mr. Bell's remarks on air-brakes seem to me to be so extremely sound that they must be paid all attention to—the air-brakes must on no account mask or blanket the controls ; they must exert their effect on the axis of the machine, and they must be strong yet large enough to produce a sensible reduction of the gliding angle. The thing can be achieved and will be a notable advance, making once again for still greater speed of flight by making such speed safer to alight from.

Mr. N. S. Percival asked whether any pilots present could tell in which direction the wind was blowing relatively to the ground without seeing the ground. He had heard it said that some pilots noticed the difference when the wind was abeam. Personally, he could not notice any difference. Also could one get any help from the wind when flying with or against it.

Mr A. F. Houlberg : Referring to the fin effect of propellers mentioned by Mr. Gordon Bell, my experiments have shown me that this is considerable. In the particular model which I have been using lately, the diameter of the propellers was nearly equal to half a span of the main plane, and they were placed right at the rear, which, in a full-sized machine, would mean having two propellers of between 12 and 16 feet behind the tail. I found that to make this model stable under all conditions was almost impossible. The reason is as follows : If a fin is placed a considerable distance in front of the machine to counteract the effect of the propellers when in flight, it is far in excess of the size it should be for gliding flight, as when the propellers stop they only offer a fraction of the area for fin effect which they do when revolving. The amount of spiral instability developed is so great that within three seconds of time when the

propellers stop the models assume an almost vertical position and spiral with the fin as centre. On the other hand, if the fin be adjusted for gliding flight, when under power the model has an excess of fin area at the rear, which is noticeable in some terrific side slips.

Mr. O'Gorman raised the question as to whether a machine climbed faster against the wind than with it. In my opinion there should be no difference if the wind is steady, but if it be gusty it is conceivable that a machine would rise faster against the wind, for every gust it meets momentarily increases its air speed, causing it to climb faster for the moment, the reverse being the case when going with the wind.

By permission from the chair, Mr. O'Gorman pointed out that the reason that Mr. Houlberg's model had been so unstable was that the propeller was so far behind the centre of gravity of the machine. Had it been placed at the centre of gravity its fin effect would have been negligible.

The CHAIRMAN, in proposing a vote of thanks to the lecturers, said that it was necessary to guard against over-confidence, as one became more proficient; confidence, skill and training were bound up together, and increased efficiency was accompanied by greater confidence. The pilot must know a large number of things to fly well, and continual practice was essential. While agreeing that knowledge of piloting had increased enormously, he thought there was no doubt as to the improvement in the machines, especially in details. One had only to visit Olympia to see this. One particular machine had gained over four miles an hour by improving details.

He recalled an early passenger flight in France in which the machine became stalled, and he remembered the pilot shouting to him to lean forward to try and get the nose down. One would not be told to do that sort of thing nowadays.

As regards engine stoppage, that mattered little nowadays, provided one was at sufficient height to be able to find a good landing. There was no doubt, however, that we needed better engines, although he thought too much attention could be given to this point, to the neglect of machines. Failure of the aeroplane itself was much more important than engine failure, and it usually meant disaster.

As regards versatility, he thought it better for military work that pilots should stick to one type after gaining experience on several.

While rain was not an insuperable difficulty, fog, darkness and clouds were formidable obstacles that had still to be overcome.

He was sure they would all wish to express their thanks in the usual manner to the lecturers for their most interesting communications. (Applause.)

Mr. B. C. HUCKS: In reply to Mr. Mervyn O'Gorman's questions:—Confidence and skill are so closely allied as to come under the one and same heading—without the one the other would be useless. The question of climbing quicker when flying into wind may perhaps be only an illusion, probably due to the fact that when flying with the wind the aircraft covers more ground in the given time, and in consequence that time may appear longer. At the same time I am under the impression that a machine does climb quicker into wind, although I cannot assign a particular reason for this.

With regard to Mr. Percival's questions as to whether a pilot could tell in which direction the wind was blowing relatively to the ground whilst flying, I find that a side wind is more noticeable on account of the increased propeller vibration; a following wind less stable than a head wind. There is also a decided difference in the feeling when gliding against wind or with it.

Mr. BELL having briefly responded, the meeting terminated with a hearty vote of thanks to Lieutenant-Colonel Sykes for presiding, on the proposition of Mr. Mervyn O'Gorman.

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The ELEVENTH MEETING of the FORTY-NINTH SESSION of the Aëronautical Society of Great Britain was held at the Royal United Service Institution, Whitehall, S.W., on Wednesday, 15th April, 1914, at 8.30 p.m. Brigadier-General D. Henderson, C.B., D.S.O., Director-General of Military Aeronautics, presided.

THE VALUE OF BALLOONING AS A TRAINING FOR FLYING.

BY GRIFFITH BREWER, A.F.AE.S., AND LIEUT. J. N. FLETCHER, R.F.C.

MR. BREWER'S PAPER.

Members of this Society may be forgiven if, in view of the glorious progress of aviation, they regard the balloon as an extinct species of aeronautical apparatus. The object of this paper is to show that, in spite of the strides aviation has made, the balloon still has its part to play in the navigation and study of the air, and that those who go to the trouble of making balloon ascents not only reap considerable pleasure by employing that means of transit, but they also learn facts about the atmosphere which may be of service to them in becoming more than mere chauffeurs of aeroplanes.

The Knowledge of the Atmosphere.

The best sailors who navigate our steamships have served their apprenticeship on sailing vessels, where they learn the true force and effects of winds and gain a knowledge of the sea not so readily acquired under steam power. In the same way, I venture to suggest that the much-despised balloon is a means for acquiring a knowledge of the atmosphere which cannot be gained when tearing through the air at fifty miles an hour.

A balloon floating freely in the air moves at the exact speed of the wind, and by observing the apparent movement of the ground immediately below, the speed of travel of the air may be accurately measured, and thus the true direction and speed of the wind at various altitudes may be noted.

The Balloon is a Good Observatory.

Complete silence and freedom from vibration enable the aeronaut to make accurate observations, whilst a sense of security due to the car being in constant stable equilibrium facilitates calm thought and appreciation of atmospheric phenomena, which cannot be so readily experienced when flying on an aeroplane.

Getting Used to the Air.

One advantage to be gained by practising ballooning before adopting aviation is that the balloonist may acquire familiarity with dangling his toes in space and steady his head in proportion to his altitude. A nervous man can, by being lifted calmly into space, become accustomed to looking down from considerable heights without that sense of nervousness which is a considerable handicap and strain to some aviators; and, having once acquired this familiarity with height, he may then undertake the lessons in control of a machine, without

the added handicap of being unaccustomed to high altitudes. Becoming accustomed to looking down from a great height is of great assistance to a pupil commencing aviation, but the main benefits derived from a ballooning training are those which give the aviator who has acquired sufficient skill to take his "ticket" the additional knowledge which will carry him beyond that standard of mental efficiency at which many aviators are content to rest. Perhaps the chief knowledge gained by the practice of ballooning, which may be of service in training a man for pioneer work in aviation, is the knowledge gained of the atmosphere in respect to the speed and direction of gusts. The probable development of weather conditions, with their bearing on the voyage to be made, and the ability or practice of keeping a position on a map without following a railway or other landmark in order not to lose his way, are also lessons learnt best in balloons.

The Aeroplane is a Bad Observatory.

The reason the air cannot be studied properly from an aeroplane is that an aeroplane must, in order to remain in the air, be travelling at a high rate of speed. Can you imagine anyone wishing to find out all about the movements of the wind confining his study to the bridge of the "Lusitania" during her passage to New York? To get any results at all he would have to make complicated calculations involving the subtraction of the vessel's speed of twenty-five knots from the result obtained by other tests, and all sorts of other errors would be obtained, including whirls and eddies set up by the passage of the vessel through the atmosphere. Compare with this the advantage the same individual would obtain if, instead of choosing the bridge of the "Lusitania" from which to take his observations, he obtained permission to sit on the top of the Eddystone Lighthouse. There every puff of wind that blew could be registered correctly in the direction from which it came, and although allowance might possibly have to be made in some conditions for the presence of the lighthouse itself, and even for the presence of the observer, yet the errors which would creep into such calculations would be infinitely smaller than those which would bustle in from all quarters on the bridge of the "Lusitania."

Piloting by Sound.

Mr. Horace Darwin has described* a most interesting theory of the way migrating birds guide their flight by the sound of distant breakers on the sea shore. In support of the theory, he kindly referred to my having heard the breakers of the sea shore on the North of France when in a balloon still over England. The hearing of those breakers is impressed vividly on my mind, because it was the only confirmation Mrs. Assheton Harbord and I had that the wind we had trusted to carry us to France was worthy of the confidence we had placed in it.

From the experience I have had of aeroplanes in flights with Mr. Orville Wright and with Mr. Alec Ogilvie, I have not been able to hear them speak to me at a distance of two feet, much less hear the breakers on the sea shore twenty or more miles away. In a balloon, the absolute stillness enables the slightest sound to be heard, and, as Mr. Horace Darwin points out, "The intensity of the sound from a single source, such as a dog barking, will vary inversely with the square of the distance, but if the sound comes from a line instead of from a point, its intensity will only vary inversely as the distance." Mr. Mervyn O'Gorman points out that this is one of the reasons why the sea breaking on the shore can be heard at such a distance. When aeroplane engines are fitted with efficient silencers, those who fly will be able to check their course and position by sound, in the way Mr. Darwin now suggests migrating birds are in the habit of doing.

* In "Nature."

XUM

An Explanation of the Ground Calm of a Summer Night.

On several occasions the practice of ballooning has suggested explanations to my mind of the cause of atmospheric movements, which, if true, must be of value to all who navigate the air. One example of this is an explanation of what happens in the atmosphere in the summer time, when, after a stiff breeze all day, the night falls with the air dead calm, the stars come out brightly, and the smoke goes straight up from the chimneys, and yet the wind continues to travel fast above. The conclusion I came to was that the air split at nightfall into upper and lower layers, the upper one continuing to travel, while the lower layer comes to rest, and in the morning the two joined again.

To support this theory, one must prove three things. First, that the wind does continue to travel above; second, that two layers can sharply separate; and, third, why they should separate and afterwards reunite.

On a clear night in the summer time, when the wind falls calm on the ground at sunset, the continued movement of the upper air cannot be observed by movement of clouds, although occasionally, when there are separated clouds, they can be observed to be travelling in the original direction. Such optical observation does not indicate the shallowness of the ground calm, which I have found on many occasions to be less than 200 feet deep.

Practical Experiences.

In proof of this, I will cite two instances.

Once, when making a balloon ascent at Battersea Park, the balloon was standing perfectly straight on the ground before we left. No sooner had we cleared the gasometer than we travelled eastward down the Thames at about twenty miles an hour. An hour and twenty minutes later we made a temporary descent at East Horndon, Essex, and we were still travelling fast when the trail rope, which was 300 feet long, had a considerable portion on the ground. When, however, the car was perhaps 50 feet from the ground, the balloon entered the calm, and we came to a dead stop. We remained in the field for the rest of the night, and when the sun came out in the morning the breeze sprang up on the ground, and we continued our excursion for another half-hour or so, when we made a landing near Rayleigh, in Essex, in a wind of not less than thirty miles an hour. The landing was a rough one, which makes it all the easier to remember.

On another occasion, going up from Wolverhampton with a hydrogen balloon just before sunset, we travelled westward at about twenty-five miles an hour, and then we came down in a field into an absolute calm. On this occasion also the calm was not reached until most of the trail rope was on the ground. We left the next morning before sunrise, and before the trail rope was completely off the ground we resumed our journey at the original speed of twenty-five miles an hour.

I could give many other instances of similar night calms on the ground and continuation of wind above, such, for instance, as most of the Gordon-Bennett Balloon Race days in September or October. From Paris, in 1906, St. Louis, 1907, Berlin, 1908, were all similar days, when we started in the evening calm, and yet travelled as fast by night as we did on the following day.

The Separation of Wind into Layers.

Now to prove my second point, namely, that two layers can sharply separate. Every balloonist of any experience must have observed that winds travel in definite layers. A wind up to perhaps 2,000 feet, blowing from the South-West, may change to West or even North-West immediately above that altitude. The two layers of air travel smoothly, with only the smallest area of

disturbance between the two layers; and when it is remembered that there are thousands of square miles over England alone of contact surface between the two layers, one must at once realise that the friction between the two layers must be infinitesimal in order to preserve their distinct directions.

I now come to the third point, namely, why the wind should fall calm on the ground whilst remaining in swift motion above.

It is obvious that wind travelling in contact with the ground must meet with considerable resistance. This is proved by the comparatively slow travel of air when measured close to the ground, and its increase in speed at higher altitudes. Consequently, the air under the influence of friction-drag caused by proximity to the earth's surface, tends to arrest the progress of the air above, and if the tenacity of the large bulk of air be sufficient to move the ground layer, the latter travels with it; if it be insufficient, then the air splits, leaving the lower portion stagnant, and the upper portion travelling at the original pace.

This would explain why they separate, but it does not explain why they join again when the sun rises.

The Action of the Sun in Mixing Layers of Air.

I assume that a constant mixing or interchange between the travelling atmosphere and the ground layer is necessary in order that the ground layer may be moved under the influence of the upper layer. At night there is nothing to cause disturbance in calm air on the ground, but when the sun comes out in the morning, objects on the ground become slightly heated, and cause bubbles of air to rise from the stagnant portion below into the portion which is moving above. A bubble of warmed air rising soon becomes a twisting column, and when you think that the proportion of thickness between the two layers of air may be likened to two table-cloths spread over a table, it will be readily realised that when some thousands or millions of these columns or bubbles are rising from one layer to the other layer, and similar quantities of cooler air are descending to take the place of the rising air, then the effect is similar to the effect of the upper table-cloth becoming pinned to the stationary under table-cloth, with the result that the two layers again move along together.

In order to show that I have not digressed too far away from my subject, I will endeavour to illustrate how an understanding of these wind conditions might have saved the lives of perhaps more than one flyer. Imagine an aeroplane coming down from a great height, head on to a wind blowing twenty miles an hour. On arriving, say one hundred feet from the ground, the machine runs suddenly into the stagnant ground layer of air, and the flying speed through the air is immediately reduced by twenty miles per hour. The machine naturally stalls and dives, and the altitude being insufficient to recover control, the machine strikes the ground before control can be recovered. Many accidents have occurred owing to the diving of machines just prior to landing when perhaps 100 feet in the air, and I cannot help thinking that wider knowledge gained in ballooning may add to the understanding of the air, and thus increase the safety of pilots when flying and prove beneficial in their training.

LIEUTENANT FLETCHER'S PAPER.

Mr. Griffith Brewer has raised a large number of interesting questions in his share of this lecture, and has covered the ground very completely. It is left to me to remark on special features of the subject in their application to military aviation, and perhaps to amplify somewhat further some of the original problems.

XUM

The Pioneer Work of Ballooning.

It will probably never be recognised how much the science of aviation owes to ballooning. Ballooning at the present day is in danger of sinking into complete obscurity, at least in this country, and I think also in France. Its use becomes less apparent every day, and I am very glad to have the opportunity to plead for it, though I cannot but regret that you should have been robbed of the pleasure of hearing Major Maitland speak on a subject on which his experience gives him the first claim to address you to-night. He has, however, very kindly allowed me to draw on his experiences and ideas.

Ballooning must be given credit for the work it has done in the past. Many of the most elementary bits of knowledge stored away in the experience of aeroplanists of to-day owe their entire conception to ballooning. The whole branch of lighter-than-air flying owes its being to ballooning. And the ground has not yet all been broken. There is a great deal of work to be done ; and many lines of study are daily opened to balloonists, and can, indeed, only be carried out by balloonists. The subject falls into two headings, viz., the value of ballooning as a preparation for flying and the value as actual training. I wish to make it clear that the first heading is a fair sub-division of the lecture. It is to my mind of the utmost importance from a Service point of view. Experience of the air we fly in is necessary. Accurate knowledge cannot be gained from mobile aircraft ; it is our object to show that it may, in part, at any rate, be gained from a proper development of the science of ballooning.

Wind Charts.

How many aeroplanists are aware of the interesting phenomenon mentioned by Mr. Brewer that the wind direction varies at different altitudes? And of those who know that the wind's direction alters, how many have tried to make use of their knowledge?

On July 13, 1912, a long-distance balloon race was held. The day was hot and thundery and the ground wind fickle. Pilot balloons sent up before the race drifted eastwards down the Thames. Those who watched the clouds saw the lower ones travelling South, but the mackerel sky above travelling North. Seven balloons went up from Hurlingham, of which four went South to Brighton at an average height of not more than 5,000 feet. The other three, choosing various altitudes, according to their ballast capacities, made Southampton, Bath, and Market Harborough respectively. The last-named must have travelled at an average height of 12,000 feet. The balloon I was in described a very marked " S," the northerly inclined limb, from south of Godalming to Fleet, being at 10,000 feet.

An aeroplane flying on that day from London to York, 200 miles approximately, could, by flying at 10,000 feet instead of 3,000, have saved about two hours on the journey. The two opposing currents were, I should say, about 20 m.p.h. at each level. The condition can be predicted by meteorologists, and it is of no small importance to aeroplanes to know and make use of it. Their endurance is limited to say five or six hours at war load. If, then, they can find favourable currents in all directions, they can save much petrol and oil, and add a considerable percentage (in the case we have considered about 50%) to the number of miles they can travel in the air. On the chance of effecting such a saving it is worth while to climb to various heights and observe progress over the ground at those heights. This can now be done very easily by means of a little camera-obscura apparatus, in which the time taken for the image of a ground object to pass between two lines can be read off directly (as a function of the height) as speed over the ground.

It might well be remarked here to those who object that too much is being demanded from the aeroplane pilot, that war is a scientific game, and must be waged scientifically. If by application of scientific methods we can increase the aeroplane's radius of action ever so little, then those methods are worth adopting; and the pilot must be trained accordingly.

Sea Breezes.

Sea breezes provide another important field of research for balloonists. On one occasion Major Maitland left London in his balloon in a flat calm. After hanging motionless for an hour or so he decided to go up higher, and went to 9,000 feet through the clouds. There he remained 1½ hours out of sight of land. I have his permission to point the moral which follows. For he then heard the hoot of a steamer, and on coming down to investigate, found the Newhaven boat just arriving, as it were, at his feet. Moral: Do not remain in balloons for more than half-an-hour anywhere in England out of sight of the ground. This is made an order for military balloons, and should never be forgotten by anyone. A record descent was made, but at about 800 feet a strong sea breeze was met, which carried him Londonwards at 25 m.p.h. Unfortunately, lack of ballast prevented him from saving railway fares to any appreciable extent, and he had to land. It would have been interesting to know where and why that breeze would have failed. Many countries in this world have nothing but sea and land breezes for six months in the year. Use could be made of them in much the same way that the barge and the steamer alike use tides to help them up and down the Thames.

These matters can only be properly investigated by balloonists, for, as Mr. Brewer points out, you cannot investigate wind strengths and directions from the deck of an ocean liner.

Suggested Scientific Organisation of Ballooning.

I should like here to plead for some kind of organisation among balloonists in this country. A few individuals make weekly ascents from London. They store up for themselves a vast deal of experience and weather wisdom, but little of this gets any further. On the other hand, most German aeronautical papers give a page or two to ballooning. A synopsis of balloon ascents for the month chronicles always some two or three hundred ascents. Another page is given to the more interesting ascents; and altogether, it is clear that an organisation exists to provide a common fund of experience from which all can draw. I would go further. I would suggest that the Meteorological Office amplify its wind chart system, and publish daily information of the upper currents, so far as they can be ascertained or predicted, and that ultimately a probability chart be got out showing upper wind directions and forces for all systems of weather. These charts would be liable to daily revision by telegraph, but they would be a great guide to pilots flying high. It is a formidable task. But if more people ballooned from more centres, and if they sent in their reports to a central registry, and if they would assist in the pilot balloon ascents made under the auspices of the I.M.A., we should, in five years, have an invaluable fund of knowledge. Night flying, fog flying, flying above clouds, would be as safe and certain as navigation by dead reckoning is at sea to-day. In five years' time aeroplanes will be so safe, and so free from engine trouble, that to "bet on one's engine running" will be to bet on a certainty. That is to say it will be possible to do all these things, which at the present time are regarded as too dangerous. But we shall then find that navigation is the difficulty. Here is, however, a little "ever ready" drift indicator, very simple and very quick to use, and involving no calculation and no drawing. From it you can read at a glance what course to steer to make good a certain course; and where you will be at any moment.

But first we must have our wind chart. I believe such a thing is possible. The lack of these charts is at the present time the great gap in airship work. We still depend on seeing either ground, or sun, or stars. Dead reckoning, so often necessary, is not possible. May I be excused here for digressing from my subject? At the present day, I think, more might be done than is done by the Meteorological Office. I am told on fairly reliable information that the Commander of a German Naval Airship which met with disaster at sea was warned by wireless from the nearest Meteorological Office of the approach of a bad disturbance, the nature of which was not quite known, and was advised not to send the ship out. The exigencies of the situation decided him to send out the ship, however, which four hours later ran into the Line-squall which overwhelmed it. No such warning is possible in England except at stated hours. The Meteorological Office issues two reports only, early morning and late evening, so that the disturbance may actually arrive before the warning of it. I suggest that warnings of unusual disturbances be sent out at once to all aeronautical centres, so that aircraft may be stopped from going out in fine weather into certain bad weather. Further, the difficulty in plotting out wind forces and directions at any part of a cyclonic disturbance, is that the Meteorological Office does not issue any definite statement of the movement of the centre of the system. Its speed and direction should always be given, and any change in either speed or direction should be sent to aeronautical centres for distribution to aircraft carrying wireless. Again, as most systems work up from S.W., warnings of actual weather conditions sent from Plymouth or Bantry would give aircraft at least six hours' warning of the weather ahead. These warnings should be sent out every three hours.

The Training of Pilots and Observers.

I must now turn my attention to the second sub-division, *i.e.*, to training pilots and observers. Ballooning might, I think, be more used for training observers. It produces quicker results in the elementary stages. A man will learn far more in three or four balloon ascents than he will in double that number of aeroplane flights. He will probably, too, reach greater heights in the balloon. The process is also attended with less actual risk and discomfort. Several ascents in aeroplanes are quite wasted, as the passenger is fully occupied in drinking in the new sensations. In the balloon, however, there are no new sensations except that of unusual calm. An instructor is always at the pupil's elbow to help him to learn, and there is no noise to prevent talking.

Aeroplanists among the audience know that cross-country flying is really very easy until trouble comes.

How many pilots leave the aerodrome for the first time full of confidence? Never before have they had occasion to watch the country for possible landing places. How many of these same pilots have landed in green corn or standing hay and smashed up simply because they could not distinguish between these features and pasture land from 3,000 feet up? How many aeroplane pilots can recognise the useless pasture that slopes like the roof of a house? The remedy I suggest is to take each would-be cross-country pilot for two balloon runs before sending him off on his own. Point out that cattle or sheep are the only safe indications of pasture; that, failing cattle or sheep, one should look out for the scar in the gateway used by animals that is not present in standing hay or green corn fields. That when cattle or sheep are feeding with heads in the same direction, it indicates either a slope or a strong ground wind; that streams or woods at the edges of fields often indicate steep slopes. These can be learnt very quickly. Five or six pounds spent on ballooning is worth far more for the experience gained, and is an excellent insurance for the preliminary risks of cross-country flying.

While on this subject, I would like to suggest that a few balloons on the strength of the Staff College would provide all Staff officers with an invaluable grounding in aerial observations and would enable them to appreciate the situation from the flyer's point of view far more sympathetically than they are likely to if they have never flown.

Finally, I cannot leave the subject of training without pointing out that the balloon training is an essential part of every airship pilot's education. He must learn to read local conditions of temperature and wind. He must be skilful in the management of his gas and envelope. He must understand, inside-out, the theory of ballooning. The bigger the airship the more he must know. Changes of temperature, up and down currents, affect airships in a way in which no aeroplane is affected, and without a thorough appreciation of the situation the airship pilot would soon be utterly fogged, and at a loss to understand the vagaries of his charge. If night flying is to be a principal rôle of airships, the pilot must be trained first in night ballooning. When real flying, away from illuminated aerodromes, becomes a possibility for aeroplanes, some regulation should be adopted to make each pilot do at least two night balloon runs before he attempts to cross unknown country by night.

Thunderstorms and Local Cloud Effects.

All aircraft avoid thunderstorms, as well they may; not a few nasty aeroplane accidents have been attributed to them, notably Captain Reynold's smash at Bletchley, where he was upset by a thundercloud at 1,500 feet. Nearly every balloonist of repute has, however, been right through more than one storm. Can nothing be done to plot out the air disturbances by means of parachutes and pilot balloons set free from the car? The danger involved is, I believe, infinitesimal, though it is not a pleasant experience.

Every cloud has its own peculiar air disturbances. The up currents in cumulus clouds are a distinct menace to unwary fliers, and measurement of their force would provide a balloonist with many hours of fascinating study.

Experiments.

Balloons provide the kick-off platform for many important experiments. Many books have been written on the subject. Perhaps one of the most interesting was Bacon's description of his sound experiments. Others are, photography, bomb-dropping, intercommunication, wireless, etc., etc., with the details of which it is not within the scope of this lecture to deal.

The atmospherical conditions over big towns and in mountainous regions require investigating. Those who have flown airships over London alone know what colossal bumps are to be met with there. It would be interesting to trace the smells of Soho to their ultimate destination!

The conclusion I hope this Society will come to is that there is a great scope for ballooning, and many problems to be worked out by its aid. There is a very serious necessity for more information about the air. The Society can assist by organising investigations, by interesting scientists and Meteorologists in the problems that present themselves, perhaps even by finding money for the work.

XUM

DISCUSSION.

Colonel J. E. CAPPER, C.B., said that he agreed with the lecturers as to the value of ballooning. The utility of that knowledge of the atmosphere which could only be obtained by balloons was very evident. Whatever might be done towards silencing the engine, an aeroplane's passage through the air would always be noisy as compared with that of a free balloon. He referred to old records made by him of changes in wind direction. He had arrived at the following rules :—

> Generally speaking, in the South of England north-easterly to southerly winds are shallow. A ground wind from the N.-E. tends to turn easterly between two and three thousand feet up; there is often a belt of calm about this height; rising above, the wind turns to S. South-westerly and westerly winds, on the contrary, are generally true up to five thousand feet. When the winds are very light they change direction frequently, often enabling complete circles to be made.

He thought the point brought out as to aeroplane accidents being possibly due to the machine having entered a calm region as it came down wanted investigating very closely indeed. He recalled an experience of Captain Waterlow, that he would like to ask him to detail for their benefit that night, in which kites had been crumpled up at a certain level by an apparent very violent change of wind direction. The effect of such a change on an aeroplane might certainly be serious. He thought that such changes could best be investigated in a free balloon, not in an aeroplane, the angle of rise of which was slight. The vertical currents which occurred in calm also wanted investigation. He would like to commend the work of the Meteorological Office, which undoubtedly did its best. He thought that much useful work might be done in the study of thunderstorms. He would certainly commend the valour of those pilots who were plucky enough to attempt this. He thought that small airships would be very useful for training observers, and he would like to see them adopted.

Captain C. M. WATERLOW : I remember the incident to which Colonel Capper referred in connection with Mr. Cody's kites. I was in charge at the time, and after two kites had collapsed at a height of about 500 feet, sent in to Colonel Capper to let him know. We then sent up a third kite, which broke up in exactly the same way as the previous two. This accident could not be put down to structural weakness of the kites, because they had been submitted to very strong winds at various times. The incident was very striking, and in a lengthy experience of kiting I cannot recall a similar case.

In thinking over the influence of balloons on airship construction, I should like to call your attention to some interesting points in connection with the first military airship in this country.

The envelope of " Nulli Secundus " was made in 1902 out of goldbeater's skin, the material used for the military balloons at that time. The first car was made of wickerwork, just as a balloon basket is, except that it was shaped somewhat like a toy boat which can be made by folding a piece of paper. This car was never actually used, though I saw it about a year ago in the yard of the Royal Aircraft Factory. The first car actually used was suspended by a net, just as balloon baskets are. The top gas valve in this airship was a standard pattern military balloon valve, and indeed this type valve has since been fitted to every military airship.

Captain Waterlow then gave a practical instance of the value of ballooning in an airship by an account of how " Beta No. 1 " broke down, and how he had to make an exposed landing on the top of a wood.

Mr. A. H. R. GOLDIE: I brought this evening from the Meteorological Office a few lantern slides bearing on the question of the diurnal variation in wind velocity. As these were not expected, we have, unfortunately, no means of showing them, but perhaps I can describe in words some of the features they were intended to convey. The true explanation of the daily variation in wind velocity—the one put forward by Mr. Brewer—is by no means new. It was, in fact, suggested as long ago as 1840 by the American, Espy. Later, in 1879, Koppen treated the question, and since then details and statistics have accumulated. These have shown that the phenomenon is much more marked in summer than in winter. The reason follows from Mr. Brewer's remarks, namely, that in summer the land by day is more strongly heated by the sun, and in consequence this upward and downward circulation of air goes on more vigorously. The diagram (Fig. A) shows this. For a similar reason the variation is more marked

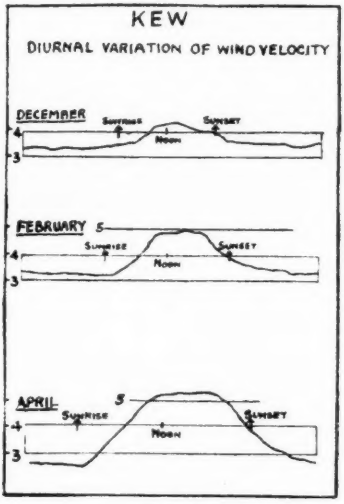

FIG. A.

on days of clear sky than on cloudy days. Again, as might be expected, the variation is extremely slight over the open sea, for here there does not exist that marked daily variation of surface temperature which is the cause of the matter.

A further point of interest arises. Since the greater surface wind by day is due to the additional momentum acquired by some of the more quickly moving air coming down from above, it follows that a kind of reverse effect is to be expected at the upper level, for this level in its turn has received a quantity of the more slowly moving surface air. Such an effect does exist, and it is commonly observed that on mountain peaks the wind is (on the average) greater by night. I have a slide here (see Fig. B) showing how the changes at the top of the Eiffel Tower (300 metres high) are in the reverse sense to those at the surface.

As to the suddenness with which the transition takes place from the night calm at the surface to the stronger wind above, I am afraid that I can say little. Generally speaking, however, it is found that the hour of maximum wind velocity becomes gradually later in the day as the height above the surface increases.

Colonel RAWSON expressed his appreciation of the valuable observations

which balloonists had collected and placed at the disposal of meteorologists. We should never have become acquainted with the dimensions of the invisible waves in the atmosphere by watching the tracks of sounding balloons with theodolites from the earth's surface. It was essential that such observations should be made from manned balloons, and though we know now that such waves measure from 200 to 500 metres from crest to crest, there are many other physical features we should be glad to know. He looked forward with much confidence to the good work which might be done by the proposed Joint Committee of this Society and the Royal Meteorological Society. The latter had arrived at the time when it believed that an analysis of all its records might be undertaken with advantage, and was now preparing to carry out this work and surrender all executive work to the Meteorological Office. It already possessed excellent charts of the upper winds which prevail in most of the weather systems, such as cyclones, anti-cyclones, etc., but it is only recently that the work of correlating had been seriously attacked. We are already learning that descending columns of air

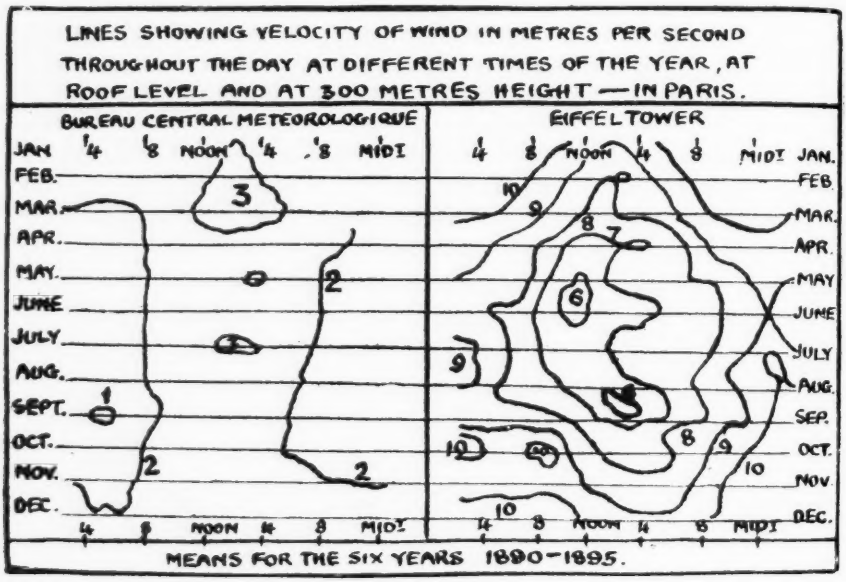

FIG. B.

must be looked for at the humps or protuberances of anti-cyclones, and not, as hitherto supposed, near the centre. Such down rushes might attain to a considerable velocity, and eddies must be expected in such a case. Mr. Cave has recently published valuable information regarding the changes of winds with altitude in direction as well as in velocity, and the lecturers will find much there that appears to have been puzzling them. Observations upon the behaviour of air-craft in an easterly wind would be welcomed by meteorologists, as much doubt prevails whether or not it really preserves the solid structure and the uniform direction in the higher levels which it is credited with. It is the only wind which directly meets the rough surface of the rotating earth, and it would be much more reasonable to suppose that it was very turbulent in places and dangerous to air-craft. He wished to thank the lecturers for the practical remarks they had made themselves and had elicited during the discussion from the reliable pilots and balloonists who had spoken.

Mr. F. K. McCLEAN said he agreed with what the lecturers had said as to the advantages of balloons for exploring air currents in most cases. There were, however, cases where aeroplanes, or at least power-driven aircraft, were necessary. He did not think sound would be so helpful in piloting as Mr. Brewer suggested, as the human ear was not capable of locating sound very accurately. As regards wind directions changing, he had had some interesting experiences in Egypt, where he had recently been. The wind up the Nile was reputed to be the steadiest of winds; it was usually north. He had noticed, however, that the upper air currents were sometimes the reverse of the lower ones, and even when a northerly gale might be blowing on the ground the upper wind still went on in the opposite direction.

They had met with vertical currents, up on one side of the river and down on the other, and for exploring these an aeroplane or power-driven craft was necessary. Where the river divided into streams and was much cut up into channels, he had found down currents on both sides of it, the up currents not being evident at all in the immediate neighbourhood.

These vertical currents were frequently quite smooth, but at the same time were powerful enough to force the machine down even when climbing steeply. Immediately over the river the air was frequently rough.

It was found that the best speed was never made in rough air, and as soon as heat dunts commenced the speed became less.

Mr. GRIFFITH BREWER: I was extremely interested in what Colonel Capper and Captain Waterlow said about the kites breaking one after the other. If I understood it aright, a pilot kite was already up highest, and as the larger kites were lifted and reached a disturbed layer of air, they broke one after the other. This is a proof of the sharp change in direction or speed which sometimes occurs in the atmosphere.

When I wrote my paper, I was under the impression that the theory I propounded of the cause for the separation of the ground wind from the upper wind and the reuniting of these layers was original. I naturally felt somewhat disappointed when I received a letter from Dr. Shaw, telling me that the same theory had been arrived at in 1840 and in 1879, and also that Mr. Dines had been making special observations on the subject which would appear in the fourth Annual Report of the Advisory Committee for Aeronautics, which will shortly be published. In propounding what one believes to be a new theory, one naturally fears that there is a possibility of the theory being wrong, or that, if right, it may not be new. It is therefore very consoling to find that the theory which I have propounded, although anticipated, appears to be a sound one, and therefore the dangers of such air conditions to flying men which I have pointed out are all the more real and serious.

Mr. Goldie mentioned an interesting point, namely, that the wind on the top of the Eiffel Tower was stronger by night than by day. I would like to suggest a reason for this. At night, owing to the small amount of friction between the upper air and the stagnant lower air, there is nothing to prevent the night wind travelling at its full speed, but when the lower air holds on to the upper air by reason of the interchanging taking place between the two layers, then the upper air feels the drag of the lower air, which it has to drag over the rough surface of the earth, and consequently the air above, at, for instance, the height of the Eiffel Tower, would naturally slow up its speed. In the daytime, therefore, I should expect a gradual increase in speed of the wind from the ground upwards, whereas after sunset in fine weather I should expect a still stagnant portion on the ground, and then a sharply defined entrance to a full speed wind which would be uniform in speed up to a considerable height.

Mr. McClean has given us some very interesting remarks on his Egyptian flying. Might I suggest that he writes up an account of his Sudan adventures during his flying to Khartoum with Mr. Ogilvie, and that he reads those adventures as a paper before the Society? The adventures he has been through remind one of the adventures we have read in Captain Marryat's books when we were boys, and good healthy adventure of this kind would be greatly appreciated by this Society.

Lieutenant FLETCHER having made a suitable reply,

The CHAIRMAN said that the thanks of the meeting were undoubtedly due to the lecturers for the most interesting papers they had read. As regards the variation of wind direction and speed in layers mentioned by Mr. Brewer, there had been many attempts at explanations, but it seemed that that put forward by Mr. Brewer was most generally accepted. It did not, however, explain all the variations in direction that had been recorded.

He wished to associate himself with what had been said in praise of the Meteorological Office, which had done a very great deal, and was always willing to help when asked. Two of its members were co-operating with the military authorities at Farnborough and Upavon, and he thought that such co-operation would go very far and produce very valuable results.

He thought that, by extending the co-operation with meteorologists, aeronauts would eventually arrive at a knowledge of the upper air such as was apparently possessed by migrating birds, and would be able to gain great assistance from making use of the fast currents up aloft. He agreed with what had been said in regard to using balloons for observation, and hoped that the Meteorological Office would be able to use manned balloons for this purpose.

He had noticed in hilly country, especially in the sub-tropics, how evident the sharp line between layers of air was, and gave examples.

As regards finding their way by wind charts and protractors, as suggested by Mr. Fletcher, he was afraid that, unless the wind charts were very accurate and revised constantly, the variations in direction and speed of the wind would make such navigation very difficult.

He agreed with what had been said as to the value of balloons for training observers, and hoped that something might again be done in this direction.

He was sure they would all wish to join in expressing their very hearty thanks to the lecturers for their papers. (Applause.)

General RUCK proposed a very hearty vote of thanks to General Henderson for his kindness in presiding, which was carried with acclamation, and the meeting terminated.

THE TWELFTH MEETING of the FORTY-NINTH SESSION of the Aëronautical Society of Great Britain was held at the Royal United Service Institution, Whitehall, S.W., on Wednesday 6th May, 1914, at 8.30 p.m. Horace Darwin, M.A., F.R.S., F.Aë.S., presided.

THE MEASUREMENT OF AIR SPEED.

BY A. P. THURSTON, D.SC., F.R.MET.S., ASSOCIATE FELLOW.

It is a matter of extreme importance to be able to measure accurately the speed of the air in any situation and under any condition, because all our knowledge of the dynamical properties of the air is dependent upon a correct measurement of its velocity, and because the safety of a pilot depends upon knowing accurately the speed at which he is flying. He is then able to guard himself against the risk of stalling his machine or of attaining too great a speed. An air speed indicator is really as indispensable to a pilot as a foot rule is to a carpenter or a "hooter" to a motorist.

The velocity of the air may be determined by three main methods.

Velocity method.

(1) In the first method the velocity is measured directly by the time taken by a particle, body or substance floating in or dragged along by the air to travel from one point to another. Under this category all rotating helical vane instruments are generally included. This may be called "the velocity method" or "v method," as the reading obtained is independent of the density of the air.

Velocity density method.

(2) In the second method the velocity is obtained by measuring the mass of air passing a certain point, as, for instance, by measuring its cooling or evaporative effect. This may be called "the density velocity method" or "the ρv method," since the mass of air passing in a given time is proportional to the product of the velocity and the density.

Velocity squared density method.

(3) In the third method the velocity is determined by placing an obstruction in the air and by measuring the force exerted or the pressure generated at the obstruction.

Suppose we consider a column of air having a cross section equal to that subtended by the obstruction and a velocity v per sec. The mass of air impinging on the obstacle per sec. will be proportional to ρv and its momentum to ρv^2. We know that force is equal to the rate of change of momentum, and since this column of air parts with its momentum to the obstacle, it follows that the force exerted or the pressure generated will be proportional to ρv^2. This

may be called "the density velocity squared method," or simply "the ρv^2 method."*

Now an aeroplane in flight derives its support from impressing a downward momentum on the air which comes within its influence. The mass of air which it engages depends directly on its velocity v and the density ρ of the air and is therefore proportional to ρv, and the velocity with which the air is driven downwards, the inclination of the machine being kept constant, is proportional to the velocity of the machine. The lift is therefore proportional to the product $v \times \rho v$, *i.e.*, ρv^2. Thus, the dynamical buoyancy of the air is proportional to ρv^2. Instruments depending on the dynamical pressure of the air may therefore be referred to as "ρv^2 instruments" or "dynamical buoyancy meters."

It is obvious that as the height increases and the density of the air decreases, the velocity must be increased to obtain the same buoyancy from the air. Instruments of this class will therefore act excellently as buoyancy meters, but will be deceptive as to speed unless the density of the air is obtained and the true velocity deduced from the reading of the instrument at that density. It should be borne in mind by pilots and designers that if a machine is travelling at a great altitude the inertia stresses caused by sharp turns or quick evolutions

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* The theory of the Pitot tube is as follows:—

The total energy E in unit mass of a fluid = the potential energy + the pressure energy + the kinetic energy.

$$= h + \frac{p}{w} + \frac{v^2}{2g}$$

Where (in suitable consistent units) h = potential height of fluid.
p = pressure.
w = weight of a column of the fluid of unit cross section and height
v = velocity.

Now in the case under consideration the potential energy is not available and therefore h = o.

$$\therefore E = \frac{p}{w} + \frac{v^2}{2g}$$

If the velocity and the kinetic energy are assumed zero, as is the case at the mouth of the Pitot tube,

$$\text{then } E = \frac{p}{w} \qquad (1)$$

And if all the pressure energy is converted into kinetic energy

$$\text{then } E = \frac{v^2}{2g} \qquad (2)$$

But the total energy is constant.
Eliminating E from (1) and (2)

$$\text{then } \frac{p}{w} = \frac{v^2}{2g}$$

$$\therefore v^2 = \frac{2g\,p}{w}$$

Let h′ = the "head" of fluid corresponding with the pressure p

$$\text{then } h' = \frac{p}{w}$$

$$\therefore v^2 = 2g\,h' \qquad (3)$$

If h″ = head of water in tilting gauge corresponding to the head h′ of the fluid.
ρ = density of fluid.

$$\rho = \frac{h''}{h'} \therefore h' = \frac{h''}{\rho} \cdot \text{ Substituting this value in (3).}$$

$$v^2 = \frac{2g\,h''}{\rho} \text{ or } v = \sqrt{\frac{2g\,h''}{\rho}}$$

The density of air is generally taken as 0.0012.

If h″ is measured in feet and g is taken equal to 32.18, then the velocity given by the above equation will be in ft. per sec.

may be greater than expected, since the velocity is greater than would appear from the instrument reading.

v Instruments.

(1) Instruments coming under the first category, such as helical vane instruments, have the advantage that they are useful as "logs" in navigation, since their readings are independent of the density of the air. If used for this purpose they should be provided with a counter to record the actual distance travelled. Thus arranged they would form an interesting and useful adjunct to the ordinary ρv^2 instrument. Instruments of the v type are not suitable for low velocities, but may be regarded as fairly accurate over the range of velocity of a flying machine. The variation of friction, due to air pressure on the vanes, which varies as the square of the velocity and the density, is however a small source of error, in view of the fact that the reading of the instrument should vary as the velocity only. In fluctuating winds an additional error is introduced by the inertia of the rotating parts making the mean reading a little high.

(2) Instruments of the Robinson cup type, in which cup vanes are mounted on the ends of radial arms projecting from a shaft which rotates about a vertical axis, appear to be difficult to class. The velocity of rotation would appear to be in part proportional to the difference in the dynamical pressure on opposite cups and in part proportional to the velocity of the wind. Experience has proved that the best approximation to the true velocity is obtained by multiplying the speed of the centre of the cups by a factor 2.2. This type of instrument appears to be regarded by the makers as of the V type.

ρv^2 Instruments.

(3) Instruments of the ρv^2 class always consist of two distinct parts, one at which the pressure is generated and the other at which it is recorded.

This class may be divided into two sub-classes.

(*a*) Devices in which the resistance of a plate or body in a current of air is measured mechanically.

(*b*) Devices in which the pressure generated on a body placed in the current is recorded on a manometer or equivalent device.

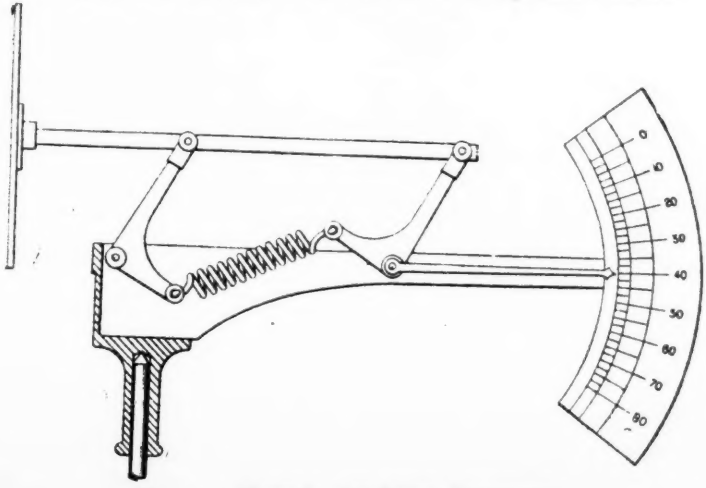

FIG. 1. MAXIM'S AIR-SPEED INDICATOR.

Type 3a.

Instruments shown in Figures 1 and 2 are examples of type 3 (*a*). Figure 1 shows Maxim's air speed indicator. In this device a circular disc is mounted so that its deflection is resisted by a spring and the amount of deflection is recorded on a dial. The dial may be graduated to record the velocity, since it varies with the resistance offered by the circular disc.

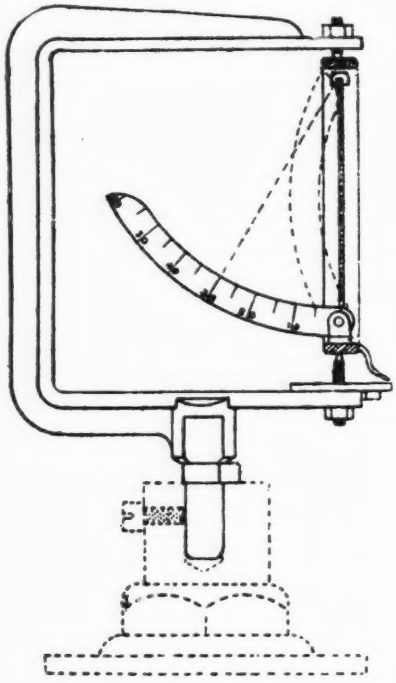

FIG. 2. SCATTERGOOD'S AIR-SPEED INDICATOR.

Figure 2, showing Scattergood's anemometer, illustrates another type in which the resistance is balanced by gravity. A plate is pivoted at its upper end and its inclination varies as the velocity of the wind. The quadrant may therefore be graduated to record the velocity.

In both these instruments, since the inertia varies as the square of the velocity, it follows that the mean reading will give the root mean square velocity which is as required. The friction at the joints, however, introduces a variable coefficient which will vitiate the accuracy of the device.

In other examples of this type of device a sphere is suspended by a cord and the speed of the air determined by the deflection of the sphere.

In another method, which is often very useful in the laboratory, the resistance offered by a body in the current of air is weighed on a delicate balance. If this resistance has been carefully recorded over a range of speeds a very accurate reading of velocity can be obtained by means of this record.

Type 3b.

Instruments of type 3 (*b*) are much more generally used in accurate scientific work, owing to their great accuracy and to the fact that the parts which are placed in the air current may be very small.

They consist of two parts.

3*b* (1) A tube for obtaining the dynamical pressure having its open end facing the wind and another tube for measuring the statical pressure of the air.

3*b* (2) A manometer for measuring the difference in pressure between the dynamical and statical tubes.

3b (1).

The dynamical or Pitot tube always consists of a tube with the open end facing the wind, but the statical tube may be of various designs. In the simplest form, with the statical tube embracing the dynamical tube, as described by Messrs. Jones and Booth,* the velocity is given by the equation

$$v = K\sqrt{2g\,h}$$

Where

v is in feet per sec.
g is the acceleration due to gravity = 32.18 approx.
h is the difference of pressure in the two tubes in feet of air at the statical temperature and pressure of the air in the tunnel.
K is a constant which may be assumed equal to unity.†

In other forms the statical tube may be provided with a hood to decrease the pressure and to give a greater reading on the manometer, as in the R.A.F. tube and the Clift pattern, shown in Figures 3, 4, 5 and 6, or it may have its axis at right angles to the current with a series of holes symmetrically placed around as in the Dines tube.

In these forms the velocity is given by the equation

$$v = K\sqrt{2g\,h}$$

Where K is a constant which is determined by comparison with a standard.

3b (2).

The pressure difference between the Pitot and the Static Pressure tube may be measured in various ways, as by—

(*a*) A liquid level gauge, such as the tilting gauge described by Dr. Stanton,‡ or by the Author,§ or it may be of the R.A.F. pattern designed by Mr. F. A. Short and described by Mr. Horace Darwin,‖ or it may be a **U** tube of the Roberts type.¶

(*b*) A cylindrical or bell float in an enclosed vessel. In this type the dynamic tube is usually placed in connection with the interior of the float and the static tube with the interior of the vessel containing the float. In the Dines arrangement the float is designed in such a way that the amount by which it is lifted is proportional to the

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* "Aëronautical Journal," July, 1913.
† "Philosophical Magazine," Vol. XXI., Fry and Tyndall "On the Value of the Pitot Constant."
‡ Proceedings, Institution of Civil Engineers, Vol. CLVI.
§ "Aëronautical Journal," April, 1911.
‖ "Aëronautical Journal," July, 1913.
¶ "Aëronautical Journal," July, 1913.

velocity of the wind. The rise of the float is recorded by an indicating device.

(*c*) A **U** tube mounted on a balance so that a difference of pressure in the two limbs causes a flow of liquid which upsets the equilibrium of the balance.

(*d*) A delicate Bourdon gauge.

(*e*) A piston and spring gauge. It is difficult in this gauge to eliminate friction. The friction causes the reading to lag behind the true reading. The readings therefore have a hysteresis loop.

(*f*) An elastic diaphragm or chamber gauge.

Types (*a*), (*b*) and (*c*) are gravity controlled devices and are particularly suitable for use in the laboratory and for delicate work, and types (*d*), (*e*), (*f*) are independent of gravity and are more suitable for use in actual flight, where a reasonably accurate "sight" reading is required which is unaffected by changes of acceleration or by shaking.

These instruments give a true mean reading if the tubes are sufficiently large to prevent friction and eddy current losses, since the inertia of the moving parts varies as the square of the velocity.

Gravity versus Spring Controlled Instruments.

An aeroplane in flight is continually subject to changes in acceleration in every direction. Now force equals mass × acceleration. If therefore the masses of the movable parts of the recording instrument are considerable or are unbalanced the readings will be inaccurate. In the case of the gravity controlled instruments it is clear that the readings will only be accurate when the machine is not subjected to an acceleration. For instance, if the machine is falling freely under gravity a small translational speed will give an infinitely large reading and conversely if the machine is being accelerated upwards rapidly the velocity recorded will be much less than the actual velocity. In turning sharply the same result will be obtained. These effects are so well known that it is difficult to realise that it required a lengthy discussion in the technical Press to establish them, but it emphasises the need for keeping the primary principles to the front. A spring controlled instrument is free from many of the limitations of gravity controlled devices, although it is not so suitable for laboratory work.

The mechanism in the human middle ear (consisting of three semicircular canals at right angles containing fluid), which gives us our "sense of balance" (Kinesthetic Sense), is obviously a gravity controlled one. It would therefore be desirable that the readings of the speed indicator should act as a check to the senses, rather that be subject to the same "illusions" due to accelerations. Obviously a spring controlled instrument fulfils this condition much better than a gravity controlled instrument.

The Clift and Ogilvie air speed indicators are described in detail because they have proved their worth and deserve to be better known. They are elastic diaphragm instruments belonging to class (*f*). Instruments of this type utilise the hydraulic or Brahmah Press principle for increasing the magnitude of a force. They consist of two parts, a pressure head and a diaphragm recording device. The Pitot tube is connected with one side of the diaphragm and the static tube with the other. The force due to the difference of pressure between the Pitot and static tubes is multiplied by the ratio of the diameters of the diaphragm and the Pitot tube. A powerful force may therefore be obtained for operating the indicating device.

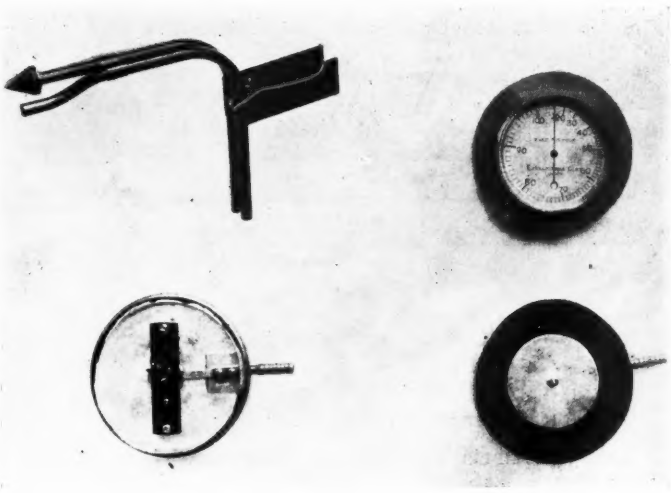

FIG. 3. CLIFT'S AIR-SPEED INDICATOR:—"HEAD" AND DETAILS.

Clift Air Speed Indicator.

This instrument is shown in Figures 3, 4, 5 and 6. The "head," which is shown in Figure 3, consists of the usual Pitot tube and a static tube provided with a conical cap to increase the suction in the lateral holes in the tube under

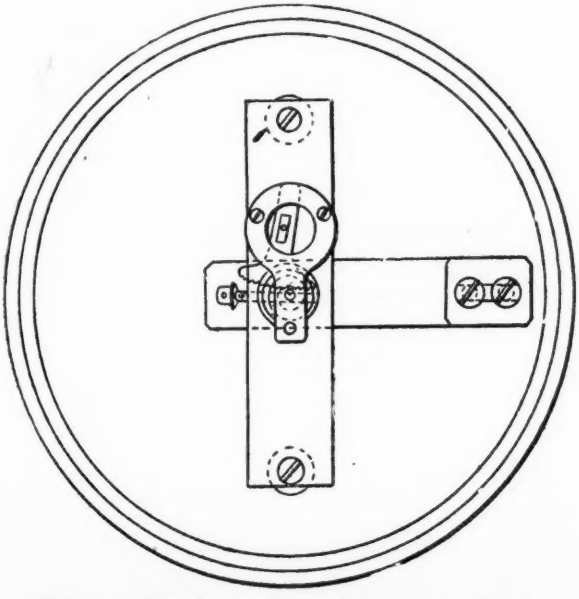

FIG. 4. CLIFT'S AIR-SPEED INDICATOR:—PLAN.

the cap. A disc of the same diameter as the base of the cone is mounted on the tube so as to leave an annular opening communicating with the lateral holes. The indicating instrument contains a cylindrical chamber divided into two parts by a very light flexible diaphragm of specially prepared leather. This diaphragm, which weighs about 1-32 oz., carries at its centre a small aluminium disc fitted

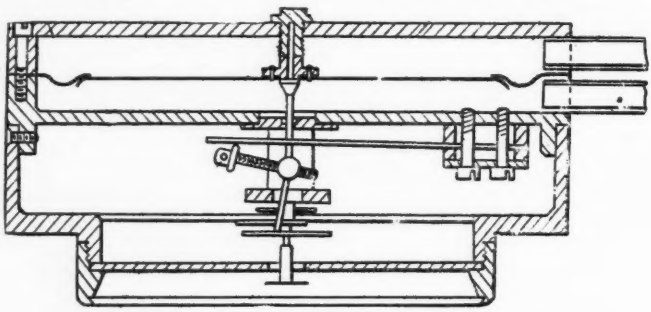

FIG. 5. CLIFT'S AIR-SPEED INDICATOR:—CROSS SECTION.

with a central boss. This boss contacts with a collar on a light spindle which is guided on both sides of the diaphragm. The upper or dial end of this spindle presses against the free end of a triangular cantilever or leaf spring. The dynamic and static tubes of the "head" are connected by tubing to opposite

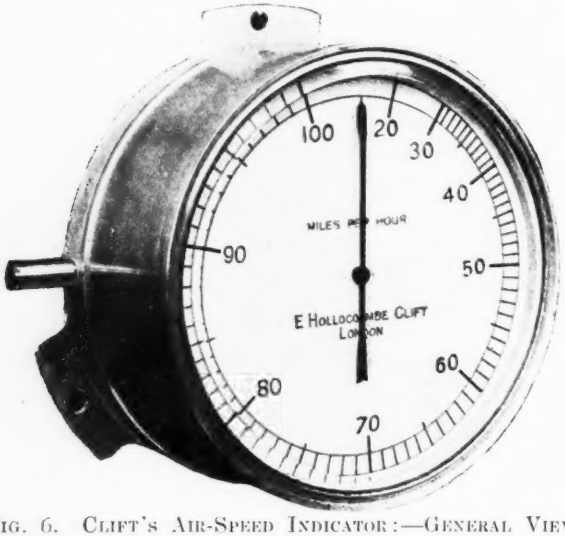

FIG. 6. CLIFT'S AIR-SPEED INDICATOR:—GENERAL VIEW.

sides of the diaphragm. Any difference of pressure between the two sides of the diaphragm is therefore transferred through the spindle to the cantilever spring which is thereby deflected. The movement of the spring is transmitted by a rocking lever having an adjustable radius arm in contact with the spring, to a multiplying mechanism, such as is used in aneroid barometers, whereby a hand or indicator is given a rotary movement over a dial which may be graduated to read either pressure or velocity. The multiplying mechanism consists of a

rocking quadrant rack which is provided with a slot engaged by a pin projecting from the rocking lever and meshes with a pinion on the spindle carrying the hand. A spiral or hair spring normally restores the hand to the zero position. The instruments are graduated individually in the works for various air pressures, which are read by a delicate tilting water gauge capable of reading to within 0.002 inches of water.

The velocity is given by the equation

$$v = 57{\cdot}7\sqrt{h}$$

Where

v = velocity in feet per sec.
h = corresponding difference of level in water gauge in inches.

The width of the annular gap has been found to affect the constant in the above equation, but as great care is taken in adjusting this width the variation of this constant is negligible with the instruments placed on the market.

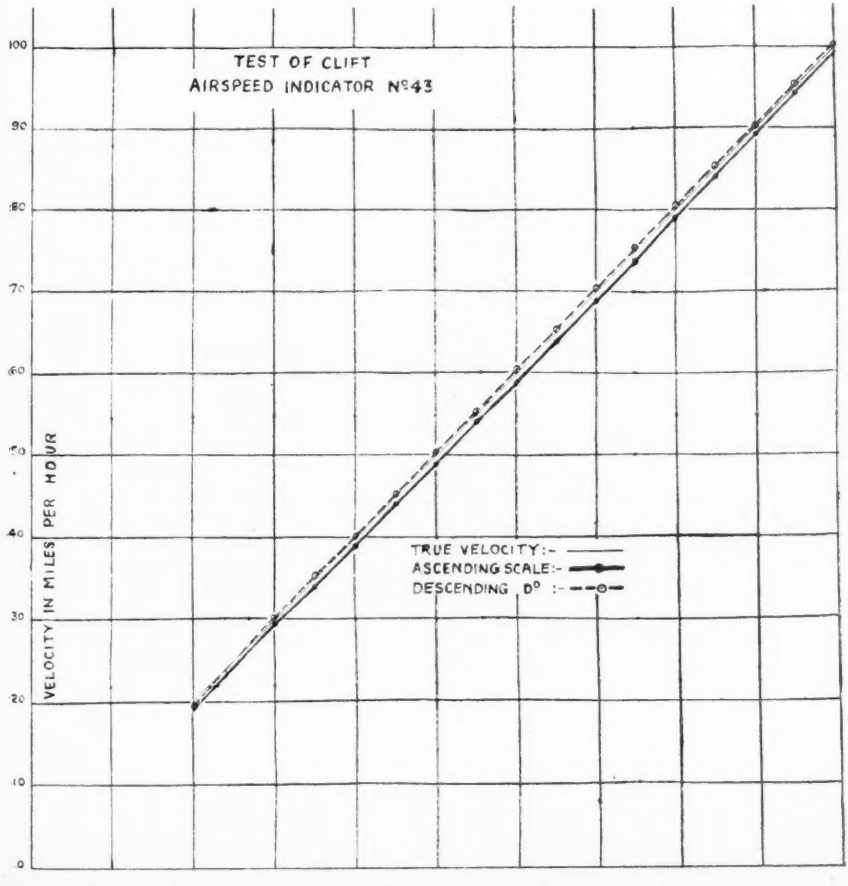

FIG. 7. CHART SHOWING TEST OF THE CLIFT AIR-SPEED INDICATOR.

These instruments have been tested between considerable ranges of temperature with satisfactory results. The inertia of the moving parts is so small that the readings do not appear to be affected by rapid changes of velocity.

In conducting an exhaustive test of any instrument the ascending and descending readings should be plotted against the true readings. Any lag due to friction of the parts, fatigue, or to hysteresis is thereby revealed. This has been done for Clift's air speed indicator and the results are plotted in Figure 7. The maximum difference between ascending and descending readings is 3%, or 1½% on either side of the mean. As, however, the greater accuracy, in flying machine work, is obviously required on the descending scale, the instrument is graduated so that the maximum error on this scale does not exceed ¼ mile per hour.

Ogilvie Air Speed Indicator.

This instrument is shown in Figures 8 and 9. Figure 8 is a photograph of the instrument used by Messrs. McLean and Ogilvie in their successful flight

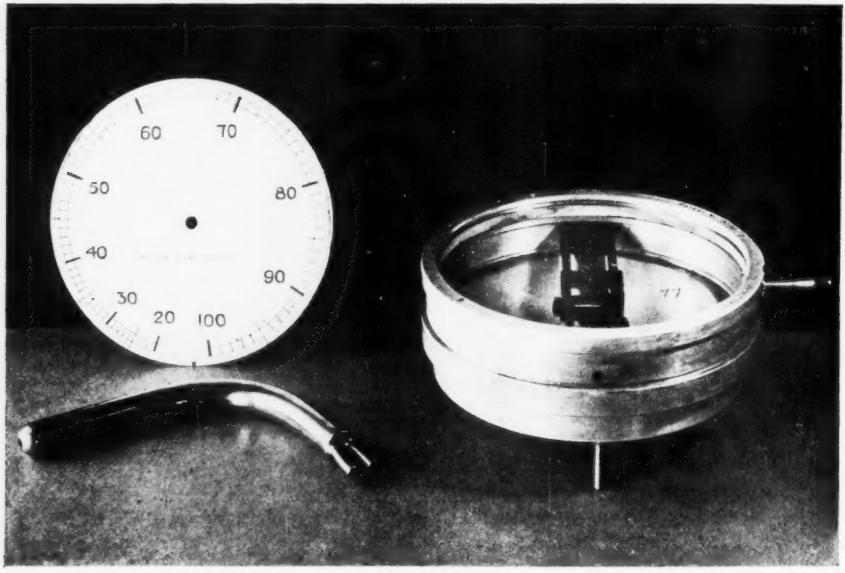

FIG. 8. OGILVIE'S AIR-SPEED INDICATOR:—"HEAD" AND DETAILS.

up the Nile to Khartoum. The Ogilvie Air Speed Indicator is characterised by extreme simplicity. A cylindrical chamber is divided into two parts by a stretched indiarubber diaphragm. The indicating hand is carried by a stout spindle which is pivotally mounted in the centre of a light metal frame bridging across the centre of the upper chamber. A second stout spindle is pivoted in close proximity

to the first spindle and with its axis at right angles to it. These spindles act as drums or rollers to a thin silk or other thread, one end of which is connected to the centre of the diaphragm. This thread (see Figure 9) passes upwards between the spindles and over the second spindle to the lower end of the indicator spindle. It then makes two ascending anti-clockwise turns on the first spindle

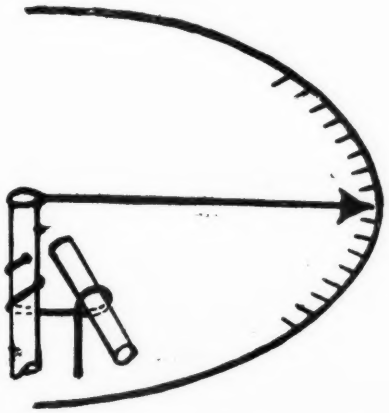

FIG. 9. OGILVIE'S AIR-SPEED INDICATOR:—DIAGRAM OF THE INDICATOR MECHANISM.

and its end is connected thereto. The head is of the usual combined concentric dynamical and statical tube type and its "constant" is stated to be unity. The dynamical tube is connected to the upper chamber and the statical tube to the lower chamber. A difference of pressure between the two sides of the diaphragm causes a pull to be exerted on the thin thread which rotates the spindles against the action of a spiral or hair spring mounted on the indicating spindle. The deflection varies with the difference of pressure and the dial is graduated in terms of the corresponding velocity. The scale, so obtained, is an "open" one between velocities of 20 and 100 miles per hour.

Hysteresis in the rubber diaphragm, causing the readings on an ascending and descending scale to differ from the true reading, is largely obviated by placing the diaphragm under an initial tension.

The instrument has been tested between the limits of 30 degrees below zero and the heat of a tropical sun and has given satisfactory readings.

There appears to be no fear that the indiarubber diaphragm will rapidly perish. Instruments which have been in use for nearly a year show no signs of deterioration. The sensitiveness of the instrument depends of course on the sizes of the piping and head. Usually 5-16 inch × 24 gauge is used on biplanes and ¼ inch × 24 gauge on monoplanes. The head shown in the photograph has a restricted nozzle, but even with this, Mr. Ogilvie says that he saw the hand jump in half a second from 55 to 69 miles an hour during a bad gust in the Sudan.

Instruments of the Clift or Ogilvie type should on no account be blown into as the zero of the instrument may be thereby upset or the indicating mechanism damaged.

XUM

ρv Instruments.

(2) This type, which has been left to the last, will be illustrated in detail by a device developed at East London College by my friend and colleague, Prof. Morris. His device has proved itself so convenient in use and so sensitive and accurate in recording changes of velocity that it can be commended to members of this Society for laboratory work.

The device can be converted into a ρv^2 instrument for acting as a dynamical buoyancy meter, but it is not proposed to discuss that question to-night.

Instruments of the ρv type consist of two parts, one part being engaged by the air and the other being connected to the first part so as to indicate the change produced in it by the current of air.

The electrical resistance of a wire varies as the temperature and various materials have different temperature coefficients.

This is shown by the following table taken from Prof. Morris's paper* which was read before the British Association at Dundee, 1912.

Material.	Melting point. Approximate. Deg. Cent.	Electrical Resistance. Temperature Coefficient. Per cent. per Deg. Cent.
Iron	1,505	0.62
Copper	1,062	0.428
Nickel	1,450	{ 0.27 commercial 0.62 electrolytic
Tantalum	2,910	0.33
Platinum	1,730	0.38
Tungsten	2,950	0.51

It will be noticed that iron and electrolytically deposited nickel have the highest temperature coefficients. (Any impurity in the nickel causes a big drop in the temperature coefficient.) If, therefore an electrically heated wire is placed in an air current its temperature will be lowered and its electrical resistance increased by the cooling effect of the air.

The following principles may be used:—

(1) The potential across the wire being maintained constant, the current flowing in the wire will give a measure of the velocity.

(2) The resistance, and therefore the temperature, being kept constant, the current will also give a measure of the air velocity.

The laws connecting radiation and velocity have been thoroughly investigated by A. E. Kennelly, C. A. Wright and J. S. Van Bylevelt, and their results are published in a paper on "The Convection of Heat from Small Copper Wires." (Transactions of the American Institute of Electrical Engineers, Vol. XXVIII., Pt. 1, 1909.) These experimenters found that

(1) The linear convection is proportional to the temperature elevation of the wire.

(2) Between 4.5 and 45 miles per hour the linear convection increases as the square root of the wind velocity.

Prof. Morris found that the most convenient apparatus consisted of a simple Wheatstone bridge made with alternate arms of a material having a high tempera-

* "Engineering," December 27, 1912.

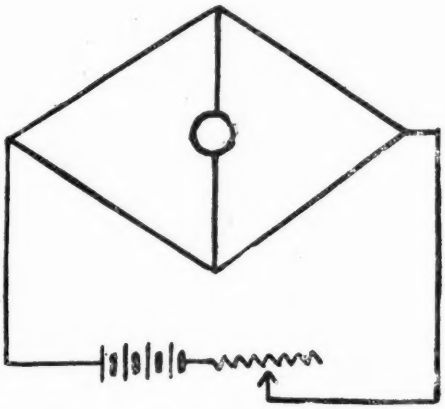

FIG. 10. DIAGRAM OF MORRIS'S AIR-SPEED INDICATOR.

ture coefficient and the remaining arms of manganin, the electrical resistance of which is practically independent of the temperature.

The arrangement is shown in the diagram, Figure 10. The potential across the bridge was maintained constant. This arrangement corresponds to the first principle enunciated above, and therefore the value of the current gives a measure of the air velocity. The sensitive arms of the bridge in one case were formed of thin platinum wire of No. 40 S.W.G. and 2.62 inches long, and the constant arms of 6 inch lengths of manganin wire of No. 30 S.W.G. This bridge could be brought into balance in still air by passing a current of 1.4 amperes through it, which gave a temperature rise of about 85 deg. Cent. in the bridge.

The battery consisted of a two volt accumulator and the millivoltmeter was a "Record" 50 ampere moving coil instrument with its shunt removed.

In another type of bridge, which is independent of the temperature of the air current, the arms consist of four iron wires, the alternate wires being shielded in tubes to form the approximately constant arms of the bridge and the exposed wires forming the sensitive arms.

This apparatus is very useful in exploring the velocity of air flow around a body. It responds rapidly to changes in velocity, and at the same time is perfectly dead-beat, and it lends itself to giving large scale indications. The velocity is read directly on the instrument and the direction is recorded by projecting the image of a vane pivoted on the sensitive wire on to a screen.

Demonstration with Apparatus.

Figure 11, which is taken from Prof. Morris' paper* on "The distribution of wind velocity in the space surrounding a circular rod in a uniform current of air," shows the results which may be obtained with his apparatus. A rod 0.825 inches in diameter was placed in a uniform air current of 15 miles per hour and the velocity at various radial distances around it was mapped out. Velocity contour lines were drawn through points representing equal velocities and the results are shown plotted in Figure 11, which is reproduced by the courtesy of the Editor of "Engineering." The velocities in miles per hour are indicated by numerals at various points. There is a "cushioning" effect in front of the

* "Engineering," August 8, 1913.

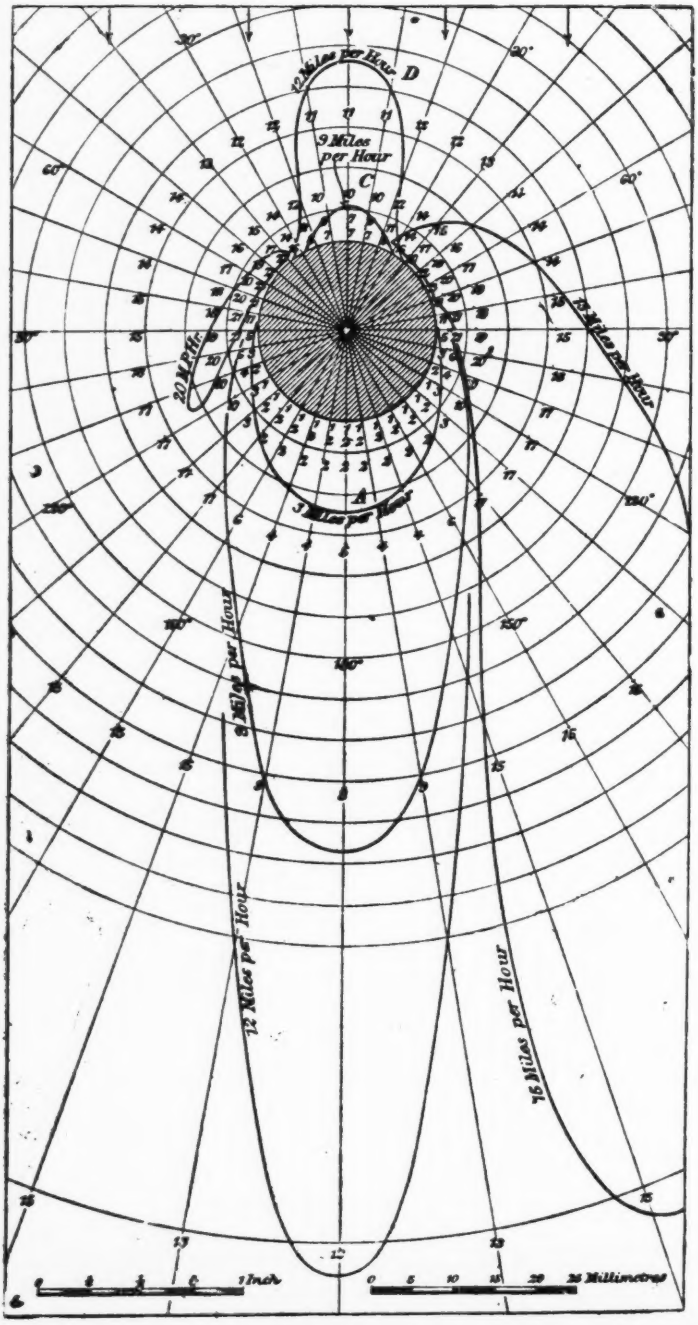

FIG. 11. CONTOUR VELOCITY LINES AROUND CIRCULAR ROD.

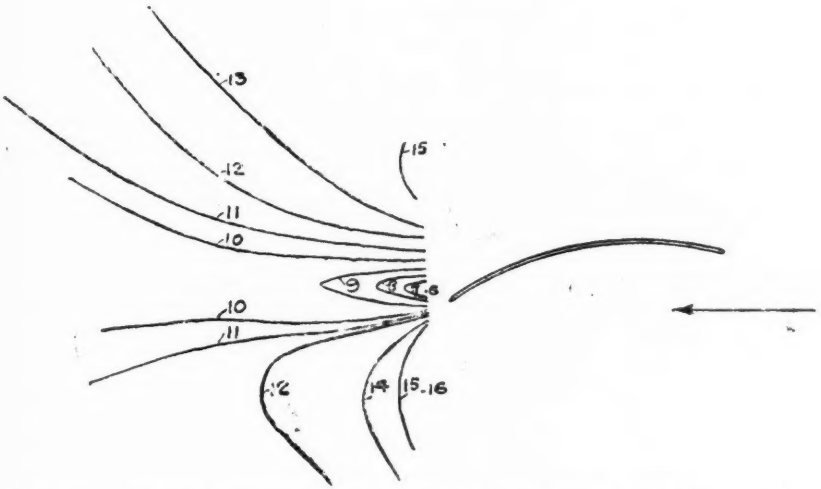

FIG. 12. CONTOUR VELOCITY LINES IN THE "WAKE" OF AN AEROCURVE AT 0°.

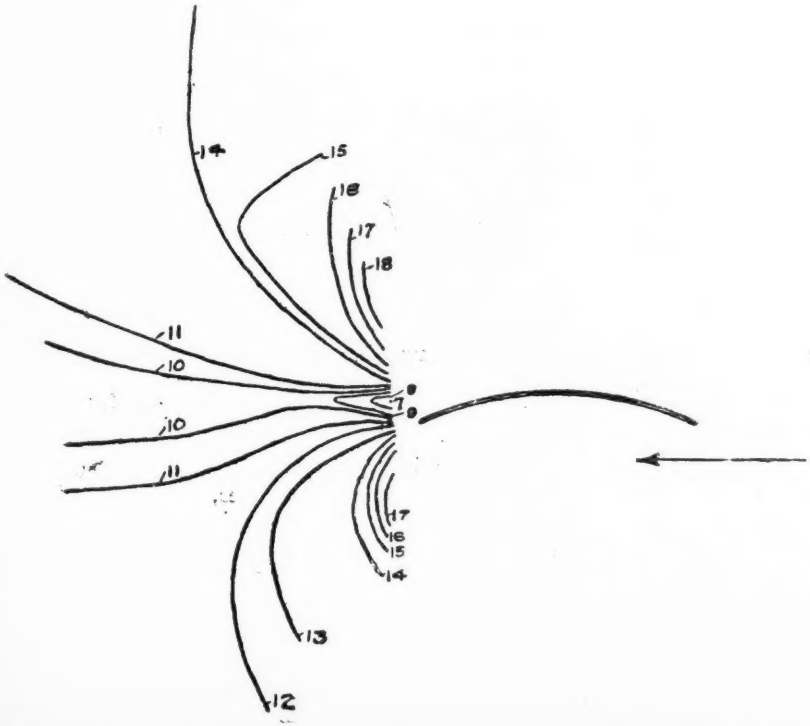

FIG. 13. CONTOUR VELOCITY LINES IN THE "WAKE" OF AN AEROCURVE AT + 10°.

rod and a "shadow" behind it. The maximum velocity is just over 23 miles per hour, or 55% in excess of the velocity of the air current, and occurs on the side facing the wind and at an angle of about 60% on either side of a line parallel to the wind and passing through the axis of the rod.

It is necessary to go at least two diameters in front of the rod to get into a velocity approximately equal to that of the air current. The shadow cast is much longer than the cushion and moreover appears to lengthen as the velocity of the air current decreases.

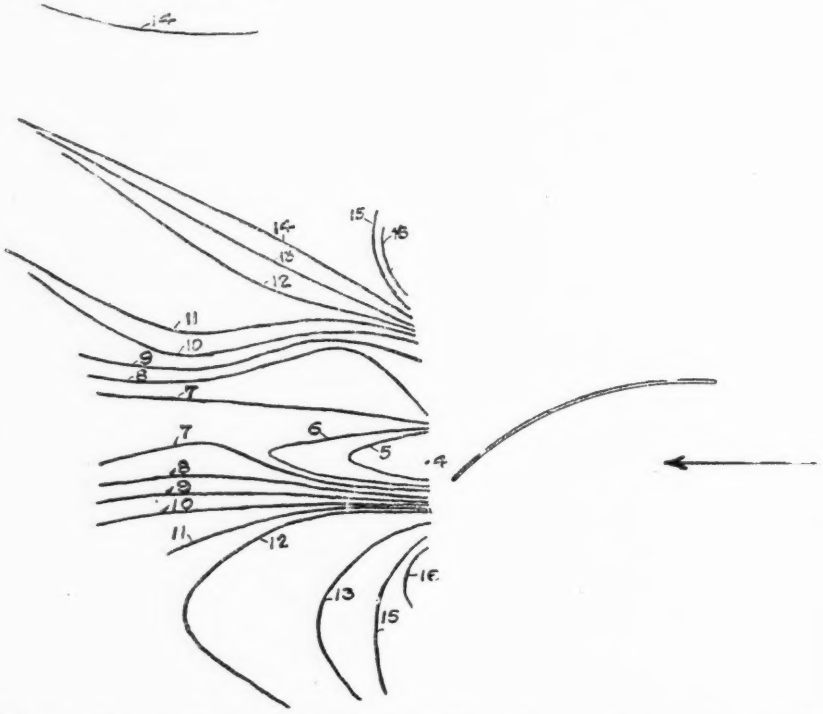

FIG. 14. CONTOUR VELOCITY LINES IN THE "WAKE" OF AN AEROCURVE AT + 20°.

This experiment of Prof. Morris' serves to indicate the need for care in placing the nozzles of indicating instruments, such as air speed indicators, so as to obviate interference due to cushioning or shadow effects.

In many cases where vertical fins, rudders, or horizontal elevators are placed in the "wake" or shadow of other bodies or planes, the results obtained may be very different from that calculated owing to the wake effect. The author has, on previous occasions and in various ways, pointed out the effect of the wake as regards stability and control. This instrument provides another means of investigating its effect.

The last diagrams, Figures 12, 13 and 14, show the contour velocity lines in the wake of an aerocurve having a camber of 1-10 of the span and set at angles of 0° 10° and 20°. It was previously used by the author for determining the distribution of pressure on inclined aerocurves and described in a paper read before the British Association, Dundee, 1912.* It is aerocurve No. 2 in that paper and has a span of 2.42 inches.

* "Engineering," September 20, 1912.

The numerals indicate the velocity in miles per hour. A section of the aerocurve set at the true inclination is shown in position in all the figures.

All the curves show in a graphic way the shadow cast by the aerocurve. It is obvious that fins or rudders placed in this shadow would not be as " efficient " in action as corresponding ones in a clear run of air.

The velocity in the immediate wake increases with the distance behind the rear of the aerocurve, but the maximum intensity of shadow appears to remain approximately in a horizontal line.

In conclusion, the author wishes to thank most sincerely Prof. J. T. Morris, M.I.E.E., and Messrs. E. H. Clift, A.F.Ae.S., F. K. McLean, A.F.Ae.S., and A. Ogilvie, F.Ae.S., for very kindly lending apparatus and supplying information. He would also record his great indebtedness to Mr. B. G. Cooper, A.F.Ae.S., for valuable assistance in obtaining information and apparatus.

APPENDIX.

Tests of the Clift and Ogilvie Air-Speed Indicators by the Author.

The Clift and Ogilvie instruments were subjected to various tests at normal temperature and also when heated up to a temperature as high as 117° F. in order to investigate their limitations.

Test 1.

The instruments were placed on a bench, free from all vibration, and subjected to an air pressure, which was very slowly increased to a maximum and then as slowly decreased. The readings of the instruments and a very delicate manometer were taken simultaneously at intervals and plotted.

The readings were found to be " studded " on either side of the mean line and the divergence from the true reading was greatest at the lowest pressures.

The divergence was greater in the case of the Ogilvie instrument than in Clift's.

Test 2.

The experiment in Test 1 was repeated, but in addition the instruments were tapped after each adjustment of pressure. When these readings were plotted against the square root of the water gauge reading they were found to lie on approximately straight lines and to be more satisfactory because they could be repeated.

It follows from this experiment that these instruments should be used in places where they are subjected to a vibration. It was noticed that the reading on ascending and descending scales fell on different lines indicating that both instruments have a " hysteresis " loop.

Test 3.

In the third test the instruments were connected together and the simultaneous readings on an ascending and descending scale were taken and plotted. The results are shown in Fig. 15. It will be noticed that the readings on ascending

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and descending scales fall on different lines. From this it follows either that one of the instruments has a hysteresis loop or that both have hysteresis loops which do not coincide.

Test 4.

The instruments were therefore tested again as in Test 2, the pressure being gradually increased up to a maximum equivalent to 100 M.P.H. and then decreased. The results are shown plotted in Fig. 16 against the square root of

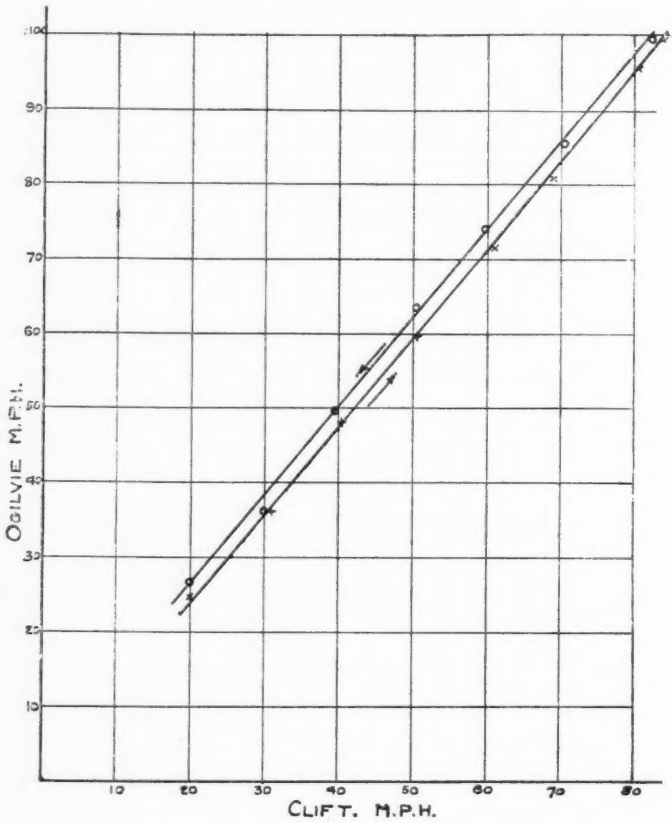


FIG. 15.

the water reading in m.m.s. At 50 M.P.H. the Clift instrument appears to give a maximum error of just over 3%, or $1\frac{1}{2}$% on either side of the mean, and at the same velocity the Ogilvie instrument appears to give an error of about $4\frac{1}{2}$%, or $2\frac{1}{4}$% on either side of the mean. The Ogilvie instrument tested was of an early type, and as the workmanship could obviously be improved it would appear that this error could be somewhat reduced.

It should be understood that, in actual practice, these errors will probably be much less because the instruments will not be required for measuring quickly changing velocities.

Test 5.

The instruments were then tested for temperature. They were totally immersed in a bath of water having an average temperature of 108° F. The readings of the instruments and of the manometer were again taken on ascending and descending scales and the results are shown plotted in dotted lines in Fig. 16.

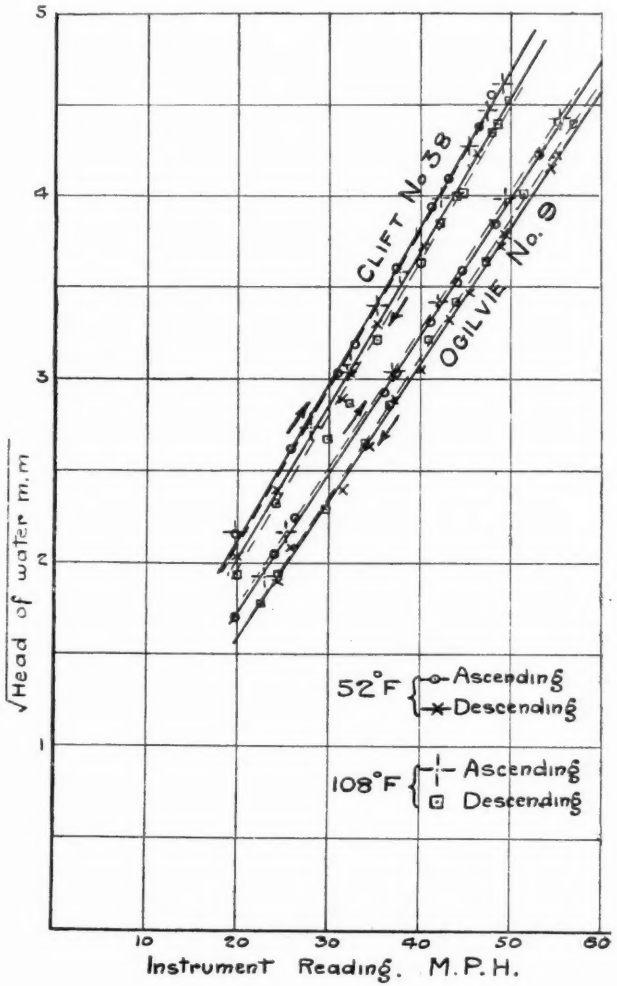


FIG. 16.

These readings do not differ very much from those obtained with the instruments cold. The maximum error caused by heating appears to be less than 1%, and may in part be accounted for by errors of observation. As far as can be seen the effect of heat is to make the Clift instrument read a little high and the Ogilvie instrument a little low.

DISCUSSION.

The CHAIRMAN, in declaring the paper open for discussion, which he hoped would be strenuous, asked some questions as to the Ogilvie air speed indicator. He wished to know how far the readings were controlled by the elasticity of the indiarubber itself; if this came in to any extent he would certainly expect a difference in reading when the instrument was heated. Had anyone shown him the instrument before it had been proved, he would certainly have thought the use of indiarubber a very grave objection, and would have said that the diaphragm would perish in a very short time. He understood, however, that it did not perish, and it was interesting to see how his pre-conceived opinion was wrong. He remarked that one could often put instruments into two classes, the first of which led one to remark, "What a fool I was not to have thought of that long ago," and the second of which led one to remark, "What a fool the man must be to think that the thing will ever work!" He was afraid that he would have put the Ogilvie instrument in the second class.

In connection with Professor Morris's instrument, he would be glad to know how the velocity indicated by the instrument was affected when the relative wind came at an angle to the axis of the wire, that is, when the wind was blowing partly along the wire. Did the instrument resolve the velocity according to some definite law, such as might have been foretold from text-books, but which would probably not happen in practice?

He was interested to see whether the diaphragm instruments would register the small amount of pull generated by the centrifugal force when the indiarubber tube connected to them was swung round in the hand. He understood one was not allowed to blow into these instruments, and this means of showing their action without blowing into them was very convenient. He demonstrated the effect by means of the rubber tube.

Mr. J. LAWRENCE HODGSON described a manometer for use in connection with Pitot tubes, which was designed to indicate wind velocities on an equally divided scale.

The instrument is shown in the accompanying photograph.

It consists of a reservoir R, containing oil, which communicates with a curved glass tube C.

The pipe P is connected to the Pitot "tip" and the pipe S to the static or the suction tube.

The special feature of the device is that the curvature of the manometer tube is such that equal movements of the meniscus along the tube correspond to equal increments in wind velocity, thus giving an equally divided scale.

Neglecting the fall of level in the reservoir R, the curve to which the tube must be bent is given by the equation

$$x = \frac{K^2}{4} \sin^{-1} \frac{2}{K} \sqrt{y} + \sqrt{\left(\frac{K^2}{4} y - y^2\right)} \qquad (1)$$

A displacer D is provided in order to adjust the zero before taking readings.

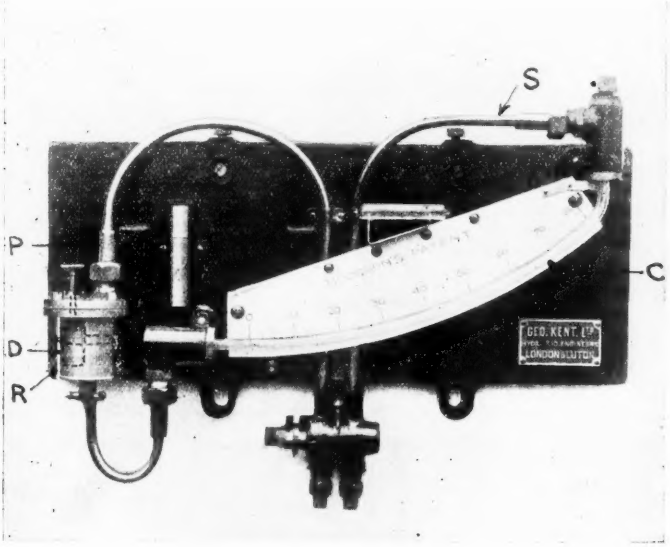
A sensitive level and means for adjusting the instrument to a horizontal position are also provided.

The scale is graduated in miles per hour. It will be noticed that the instrument measures the wind velocity over a very large range.

In practice the manometer tube is bent so as to fit as nearly as possible the templet which has been calculated from equation (1).

The actual scale is then obtained by calibrating the instrument against a very sensitive precision manometer.

In this precision instrument the differential pressure is read to 1/2000 of an inch by means of a travelling microscope.



In reading the height of a fluid column to this accuracy the greatest possible care has to be taken with regard to the illumination of the meniscus. This is best done by shielding the end of the microscope with a blackened tube and letting light pass only parallel to the axis of the microscope. In this way a very definite and black meniscus is obtained.

For accurate work oil of known specific gravity is preferable to water, as with oil the meniscus is always of regular shape.

Mr. Hodgson also showed slides illustrating wind velocity recorders and various air metering devices.

Mons. A. DELAUNAY: The principle of Professor Morris's apparatus, namely, the change in electrical resistance of wires with temperature, can be used for designing an apparatus which does not require a graduated dial and gives a record of the experiments.

If instead of a Wheatstone bridge and a voltmeter, we use two wires placed at some distance apart (see Fig. A), each one being part of the circuit of a recording amperemeter (the two recording devices being driven synchronically) (see Fig. B), we shall get two diagrams. If a gust reaches the first wire, the change in resistance of this wire will be recorded by a bend in the diagram line. When the gust reaches the second wire, the change in its resistance will be recorded in the same way on the second diagram.

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Then it will be possible to know the time necessary for this gust to travel between the two wires. The distance apart of the wires being known, we shall have the mean velocity of the gust between them.

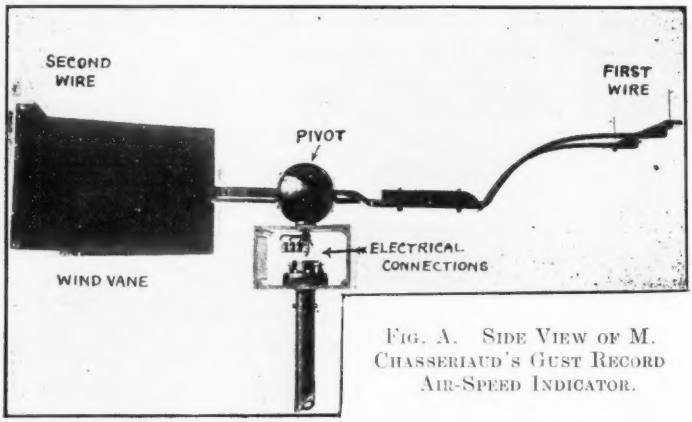

FIG. A. SIDE VIEW OF M. CHASSERIAUD'S GUST RECORD AIR-SPEED INDICATOR.

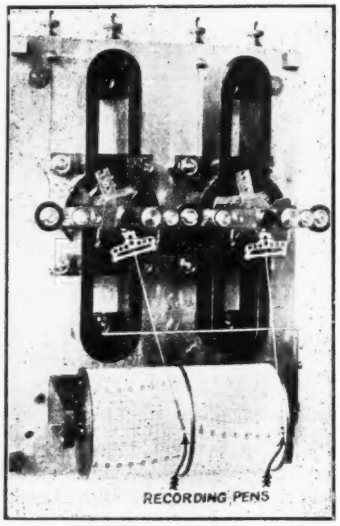

FIG. B. THE RECORDING MECHANISM OF M. CHASSERIAUD'S GUST RECORD AIR-SPEED INDICATOR.

If the distance is large, this velocity will be the propagation velocity of a wave, but if this distance is short, it will be really the actual velocity of the gust at a given moment.

The chief objection to this method is that it will perhaps be difficult to separate the loops corresponding on each diagram to the same gust. I can only say that experiments made in the open air have shown that it was perfectly possible to identify on both diagrams a gust of some importance.

In fact, this instrument, described by the inventor, Mr. E. Chassériaud, at the Aeronautical Congress of Turin (1911), does not measure the true velocity of the air, but the changes in its velocity which are, by far, the most important data in the questions regarding the stability of aeroplanes and the stresses on their planes.

This apparatus has proved very sensitive; changes in velocity of 2 to 3 centimeters per second (100 yards per hour) have been recorded. With this instrument, for example, Mr. Chassériaud could record the waves created in a large room by opening its door. It has been used for recording the movement of one pendulum oscillating in the air. It can be used not only in laboratories, but in the open air.

Regarding the use of the Pitot tube on flying machines, it has been found in the experiments on full scale machines carried out at the Aeronautical Laboratory of Saint Cyr, that the air is disturbed far ahead of the wings. Aviator Lieutenant Gouin, who has been engaged in these experiments for more than one year, was obliged on a single-seater Bleriot to place the end of his Pitot tubes at a distance of five feet in front of the wings, although the tubes were far enough from the propeller not to be disturbed by its stream.

Mr. E. Hollocoombe Clift said that as regards blowing into his gauge, the effect was not so serious as might be imagined, and he had designed a form which could be blown into quite vigorously without damage. He had a specimen present for those who wished to see it.

He had found that alleged defects of air speed indicators, in giving wrong readings of speed, were often due to the fact that the Pitot head was not placed in a position where it was free from the air that had been affected by the passage of the aeroplane. He had found it necessary to place the head some distance in front of the machine to ensure immunity from interference. It was, of course, possible so to place the Pitot head on the machine that the air speed was consistently over-stated by a considerable percentage, and some makers of aeroplanes would no doubt welcome gauges that gave such speed readings for their machines!

Mr. F. H. Bramwell: I do not propose to offer much criticism of Dr. Thurston's paper; but, having spent a considerable time during the last few years in determining a standard of velocity for aeronautical work, I may perhaps be able to add a little to the knowledge of this subject.

Dr. Thurston has made a statement in his paper that "v" anemometers are of no use for measuring low velocities, thereby implying that "v^2" instruments are better; but from the very nature of the indications it can be seen that "v" instruments are better than "v^2" instruments for the measurement of low velocities. During the course of my work, I made a vane anemometer that would read velocities of about 0.5 feet per sec.; this was only attained by reducing the bearing friction by the use of jewel bearings, and also by the elimination of all gearing, using an electrical method for indicating the revolutions. Of course, at such low speeds, the reading per second of the instrument was not proportional to the velocity, but could be very well expressed by the relationship:—

(Reading per sec. + a constant) proportional to (the velocity).

I would also like Dr. Thurston to draw a very sharp distinction between tubes that register the true static or atmospheric pressure and those that measure a suction; the two names, static pressure and suction tube, are sufficiently descrip-

tive of the two types, between which there are two important differences, which I will attempt to illustrate.

In determining our standard of velocity, I used a whirling table method, by which the actual speed was obtained by measurements of time and distance; by making an independent measurement of the velocity of the air set in motion by the movement of the whirling arm, and performing the experiments with every care, I was able to measure the velocity through the air to an accuracy of one-tenth per cent., *i.e.*, one part in a thousand. Using this method, it was found that every shape of Pitot tube, ranging from a hole in the centre of a flat plate to a thin walled tube only one millimetre in diameter, gave a pressure of $\frac{1}{2}\rho v^2$ to the degree of accuracy of the experiments. So that the problem resolved itself into the determination of a form of tube that would register the atmospheric pressure. It was found that a tube parallel to the wind direction, and having circumferential rows of small holes at least four or five diameters from the end, did so to the degree of accuracy of the experiments. The great advantage of such a tube is that the correct reading is always obtained in spite of indifferent workmanship; whereas a tube that measures a suction has to be made with the greatest care and even then there is a great liability of differences obtaining between apparently similar tubes.

This is one big difference between the two types of tube, the other is that the indications of a combined Pitot and static pressure head are proportional to the square of the speed within the accuracy of measurement; whereas, in the case of all tubes measuring a suction, the indications are not strictly proportional to the square of the speed. The departure from the square law is greatest at low speeds, say, up to 20 feet per sec., so that for use on aeroplanes this difference is of no importance; but as a standard of velocity the advantages of the combined Pitot and static pressure tube are obvious.

The suction tube has two advantages for use on aeroplanes, one is that the pressure differences registered by the instrument are greater, the other is that the tube is not quite so sensitive to small changes in the wind direction. A combined Pitot and suction tube can probably be used in winds inclined at 15°, whereas the indication of the static pressure tube will start to alter at half this angle.

Where very rapid changes of wind direction are experienced, as in meteorological work, it is probably preferable to use a Dines tube, which is not affected at all by changes in the wind direction.

For experimental wind tunnel work, a big advantage is gained by determining the velocity from the pressure difference between the inside of the tunnel and the outside atmosphere; this is measured by connecting one end of the manometer to the outside atmosphere and the other end to a small hole in a plate flush with the inside of the tunnel. The reading of the manometer is then calibrated against the velocity in the tunnel by placing a standard Pitot and static pressure tube in the position that is to be occupied by the model under test. This tube is then removed during the actual tests, leaving the tunnel quite free from any obstruction.

The Pitot and static pressure tubes can also be made in a size suitable for velocity exploring and plotting; in one such tube, that has been used for this type of work, the diameters of the Pitot and static pressure tubes were less than one millimetre; such a tube probably has less interference with the flow than any other type of velocity measuring instrument, and could, if necessary, be fitted with a small vane to keep it parallel to the wind direction.

Lieutenant-Colonel R. DE VILLAMIL (*communicated*): In Dr. Thurston's paper there are one or two points which appear to call for comment, part of it being

based on loose argument supported by mathematics, which certainly lead to very curious results!

Under the heading "Velocity Squared Density Method," he says, "Suppose . . . a column of air having *a cross section equal to that subtended by the obstruction* and a velocity v per sec." The mass of this air is certainly *not* ρv *(unless the size of the cross section is unity):* making this supposition its momentum—*passing a certain point per second*—will clearly be ρv^2: but this mass of air does *not* "impinge on the obstacle"—though a *part* does. It does *not* "part with its momentum to the obstacle"—but only with a moiety, at most. The pressure will therefore *not* be ρv^2, but $\frac{1}{2}\rho v^2$. If Dr. Thurston's argument were sound the air would have to stick to the obstruction or disappear into some mathematician's "sink"—neither of which is usually found to be the case.

In the mathematical note to this, which is called the "theory of the Pitot tube," we are told that the total energy = potential energy + pressure energy + kinetic energy. This is, of course, as obvious as that 6 = 3 + 2 + 1. I might in parenthesis point out that energy is recognised in only *two* forms, kinetic and potential: "pressure energy" is as much "potential" as what I suppose is intended for "energy of position."

In following Dr. Thurston's argument I will change some of the names of the terms, thus: Breakfast = tea + bacon + eggs. (I might add a few more items to the list.)

Now in the case under consideration the tea is not available (query: tea-pot "broke itself") and therefore tea = 0.

∴ Breakfast = bacon + eggs.
If there are no eggs, then breakfast = bacon ... ... (1)
Also if there is no bacon, then breakfast = eggs ... ... (2)
From which it is easy to show that bacon = eggs.
(A result which is a little surprising).

If, however, we substitute the values of bacon and eggs from formulæ (1) and (2) in the original formula, we get breakfast = breakfast + breakfast, or 1 breakfast = 2 breakfasts.

But for the unfortunate accident to the tea-pot, we should have had 1 breakfast = 3 breakfasts.

Further, if a few more items had been added to the menu (butter, toast, etc.), we might even get: One breakfast = *several* breakfasts.

Referring to Fig. 11, the curves of which are so wonderfully regular as to excite the very gravest suspicion, much might be said; I will confine myself to one single point, which bears on what I have previously discussed.

We see that at the rear of the rod the velocity has been reduced from 15 to 1. The kinetic energy has been reduced—*and the pressure increased*—by an amount which can be measured by $(15\text{-}1)^2 = 196$. This pressure, also, occurs over an area which extends over an arc of more than 120°.

At the front of the rod the velocity has been reduced from 15 to 7 only. The pressure has therefore been increased by an amount which can be measured by $(15\text{-}7)^2 = 64$. This pressure occurs over an area of *less than* 80°. It would appear, therefore, that we have here a very fine example of a "perpetual motion flow"!

It may be objected to me that Dr. Thurston's previous experiments have shown that the pressure at the rear of the rod is not increased, but *decreased;*

other experimenters have noticed the same thing. Such being the case, we have the kinetic energy here, almost reduced to *zero*, whilst the "pressure energy" has also been very considerably reduced. I should like to know what has become of all this energy. Has it disappeared into some mathematical "sink"? If not, is Dr. Thurston quite sure that what he was measuring was actually the kinetic energy of the fluid?

Dr. A. P. THURSTON: Mr. Horace Darwin has raised some pertinent questions with regard to the Ogilvie and Morris air-speed indicators. The use of india-rubber for the diaphragm of the Ogilvie instrument strikes everyone as a daring experiment, but the proof of the pudding is in the eating, and Mr. Ogilvie tells me that the india-rubber diaphragm has been found to give good results over a period of a year. The diaphragm is placed under an initial tension and the hysteresis of the instrument may be decreased by increasing this initial tension. A small spiral hair spring on the indicator spindle returns the pointer to the zero position and places a small additional tension upon the diaphragm. How far the hardening of the diaphragm with age affects the graduation of the instrument, if it does so at all, I do not know, but I should like to test an instrument over a long period in order to investigate the matter.

The reading given by Prof. Morris's instrument does not vary when the wires are inclined to the air, at least up to angles of 45°. The wires are mounted between baffle plates, which guide the air between them, and in the tests made in a wind tunnel no difference whatever could be detected in the reading as the inclination was varied. This is a strong point in favour of the instrument, particularly for experimental work, in which the direction of the wind may be in doubt. Mr. Lawrence Hodgson's remarks are interesting. The method suggested of curving the tube of the manometer so as to obtain equal movements of the liquid for equal changes of velocity is a very useful one. I have seen instruments embodying this principle in use for many years and I believe the principle has been employed by Mr. Dines, but I have not had time to verify the matter. Perhaps Mr. Hodgson will be good enough to do so.

I have heard of M. Chasseriaud's air-speed indicator mentioned by Mons. A. Delaunay, and welcome the additional information now received. The instrument will probably be of great use for certain types of experiment.

The air is undoubtedly disturbed far ahead of the wings, and the correct positioning of an air-speed indicator on a flying machine requires careful consideration and experiment. If the instrument is badly placed, it may be necessary to apply a correction to the readings. The ideal condition would, of course, be to graduate the instrument in position on the machine. Doubtless some makers would welcome gauges which gave an exaggerated speed reading, but they would be disillusioned in the first competition.

I agree with Mr. Bramwell's criticism. My remark as to "v" anemometers was directed against those on the market. Vane anemometers may be made for use in the laboratory—as was done by Dr. Stanton—for reading very low velocities with great accuracy, but the limitations of these instruments are obvious, particularly in gusty or variable direction winds.

It is certainly desirable to draw a sharp distinction between static tubes *per se* and those which, in addition, measure suction.

It is interesting to note from Mr. Bramwell's experiments that every shape and size of Pitot tube gave a pressure equal to $\frac{1}{2}\rho v^2$. This is a little surprising, since it is known that a large normal plane gives a higher resistance coefficient than a small plane, and since it is known that the pressure falls off at the edges and in some cases may become negative. It would therefore appear probable, with very exceptional sizes and shapes of Pitot tubes, that a small deviation from the $\frac{1}{2}\rho v^2$ law might be found.

The method of measuring the velocity of the air in a wind tunnel by means of a suction hole in the side of the tunnel was discussed at East London College when the first tunnel was suggested. The method has been used for many years for measuring the velocity of fluids in pipes. It was not adopted at the College because of the convenience of the Pitot tube and because it was feared that the reading would be affected by the body placed in the tunnel. These fears, however, have been proved by Dr. Stanton and Mr. Bramwell to be groundless, and acting on information kindly supplied by Dr. Stanton, we have tried the method and found it to give very satisfactory results. We found, however, with large bodies placed at excessive angles in the tunnel that the reading of the suction hole could be influenced. Care should therefore be taken in using this method.

Combined Pitot and static pressure tubes fitted with a vane may be used for exploring velocities around various bodies, but the method is slow and cumbersome in comparison with Prof. Morris's method.

Lieutenant-Colonel de Villamil's remarks are in part the result of the condensed form of the references to the flow of air around a body. Our knowledge of this flow at various velocities and around various bodies is still so imperfect that this portion of the paper was purposely condensed in order not to stir up irrelevant contentious matter. The remarks criticised are correct and have been amply proved by experiments. It should be pointed out that the mass of the air is not stated to be equal to ρv, but *proportional to* ρv, which is a very different matter. The same remark applies to the momentum.

Problems relating to the flow of fluids may be treated in two ways: (1) By the careful experimental investigation adopted at the National Physical Laboratory, and this is the method which I should adopt; or (2) by clearly defining the premises and then investigating the problem mathematically as in Prof. Lamb's beautiful manner.

The form of the equation for the energy in a fluid is criticised, but it should be stated that this equation has been used by hydraulic engineers for generations, and, because of its convenience, there appears to be no reason why it should be modified.

As regards Colonel Villamil's mathematical breakfast, I am afraid that the premises are incorrect and do not require discussion.

Fortunately for me I had no connection whatever with the experiment described with relation to Fig. 11 beyond suggesting that the experiment would be an interesting one. Prof. Morris is well known as a careful experimenter, and I think his results may be accepted.

The CHAIRMAN having proposed a vote of thanks to the lecturer, which was carried unanimously, Dr. E. H. HANKIN proposed the hearty thanks of the meeting to Mr. Horace Darwin for his kindness in presiding. He (Dr. Hankin) was on leave from India, and therefore, in the position of an onlooker, and speaking from that position he wished to say how much he enjoyed the Society's meetings. The keenness of the speakers and the value of the discussions was most marked. (Applause.)

The CHAIRMAN thanked the meeting for their appreciation, and the meeting terminated.

THE THIRTEENTH MEETING of the FORTY-NINTH SESSION of the Aëronautical Society of Great Britain was held at the Royal United Service Institution, Whitehall, S.W., on Wednesday, 20th May, 1914, at 8.30 p.m. The Right Hon. Lord Sydenham, G.C.M.G., F.R.S., presided over a large and distinguished audience.

The CHAIRMAN said that he wished to congratulate the Aëronautical Society on their decision to commemorate the work of Wilbur Wright by an annual lecture.

No more appropriate memorial could be suggested than this meeting to record progress, and to diffuse scientific knowledge as a help to future advances.

It is to the eternal honour of the Wrights that with very small means and by real hard work they succeeded when several earlier experimenters had failed. They showed that an aeroplane could be balanced in flight. They gave a new impetus to aeronautics, and the wonderful progress which we have seen since their first flight in 1903 is due in great measure to their courage, persistency, and sound scientific perception.

Dr. Glazebrook needs no introduction from me. He is the head of the National Physical Laboratory, which has done some admirable aeronautical research work, and he is Chairman of the Advisory Committee which is placing all the latest developments of the science at the disposal of the Government.

The lecture which he will give us will add materially to our knowledge of some fascinating problems and will help to promote the important objects for which the Aëronautical Society exists. (Applause.)

THE DEVELOPMENT OF THE AEROPLANE

BEING

THE SECOND WILBUR WRIGHT MEMORIAL LECTURE.

BY DR. R. T. GLAZEBROOK, C.B., F.R.S., FELLOW.

I am honoured by the request of your Council to deliver the second Wilbur Wright Memorial Lecture, and for my subject have selected some points connected with the development of the aeroplane, that machine whose capabilities Wilbur Wright was among the first to recognise and which he did so much to create.

I will commence with some well-known words:—

"I have brought to a close the portion of the work which seemed to be specially mine, the demonstration of the practicability of mechanical flight; and for the next stage, which is the commercial and practical development of the idea, it is probable that the world may look to others. The world indeed will be supine if it do not realise that a new possibility has come to it and that the great universal highway overhead is now soon to be opened."

These words were written in 1897 by S. P. Langley, Secretary of the Smithsonian Institute, describing his experiment of May 6, 1896, when he launched a heavier-than-air machine which flew under its own power (steam) for about half a mile. Langley's experiment had no small influence on the work of

the Wrights. Let us take it as our starting point, and as an introduction to our subject, let me show some slides, most of them well known, which will illustrate the progress that has been made.

Early Efforts.

We commence with Henson's aeroplane, designed in 1842 and never constructed, and Ader's, who in 1897—the year after Langley's experiment—succeeded in traversing without entirely leaving the ground short distances in a machine weighing 1,100 lbs., driven by a steam engine of 40 h.p., weighing 7 lbs. per h.p. Fifty years before this Stringfellow, in 1848, had constructed a small model which flew some 40 yards under its own steam, while in 1868 he exhibited at the Crystal Palace a triplane shown in the next slides. The next slide shows Langley's aeroplane of 1896. Then we have Langley's quarter-size model of 1901; in this he had Manly's help. Langley's large aeroplane, 1903—the same year in which the Wrights made their first flight at Kitty Hawk with a machine weighing 750 lbs. and a 16 h.p. motor; Ferber in 1903; Phillips, 1904; Wright's machine of 1905; and Santos Dumont of the following year, 1906.

Development since the Wrights' First Flight.

Let us pass on to more modern days. It is only some ten years since Wilbur Wright and his brother flew in their first machines at Kitty Hawk—six or seven years since they astonished the world by the success of their flights in France, and yet how great has been the change! Compare the machines, of which illustrations have just been shown, with those exhibited some six weeks since at Olympia. Development due to the labours of many workers, the thoughts of many minds, the skill and experience of pilots in many lands, was shown on every side. The task of tracing in all its details the growth of that development is too heavy for a single evening, and indeed, if I could attempt it, I could not make it interesting. I wish to limit myself to one corner of the field—perhaps the title of my lecture is too wide—I propose to deal mainly with the work of experiment and scientific research in the development of the aeroplane. Laboratories for the study of aeronautical problems exist in many places and each has contributed to our present knowledge. No small share belongs to the National Physical Laboratory, where Dr. Stanton, Mr. Bairstow, and their colleagues have worked with so large a measure of success, and if to-night I am able to put before you any new facts connected with our subject, it is solely due to the zeal and skill with which the Laboratory Staff have attacked and solved the complex and most interesting problems proposed to them.

The experiments I propose to consider are all conducted, as you know, in an air channel. A model of the aeroplane or the part of the aeroplane whose behaviour it is desired to study is supported in the channel on the arm of a balance by means of which forces and moments acting on it when a current of air is produced in the channel by a suitable fan can be measured. I will not attempt to describe the mechanism—many of my hearers have seen it. The velocity of the air current is measured by a Pitot tube and the precautions taken have secured a constant distribution of velocity across nearly the whole of any section of the channel.

We wish, however, for information as to actual aircraft, not merely models, and our success in obtaining this will depend on the accuracy of our measurements and our knowledge of the law which connects the forces on the model with those which act on the aeroplane itself.

As to the accuracy of the results—there are now two channels, one 3 ft. square, the other 4 ft. square, in daily work; a third large channel, 7 ft. square,

XUM

is nearly complete. Recently the lift and drift of the same aerofoil were measured by different observers in the two channels. The results are shown in Fig. 1, those in the 4 ft. channel being indicated by a **X**, those in the 3 ft. by a **O**, the two series are practically identical; the accuracy attained is very high.

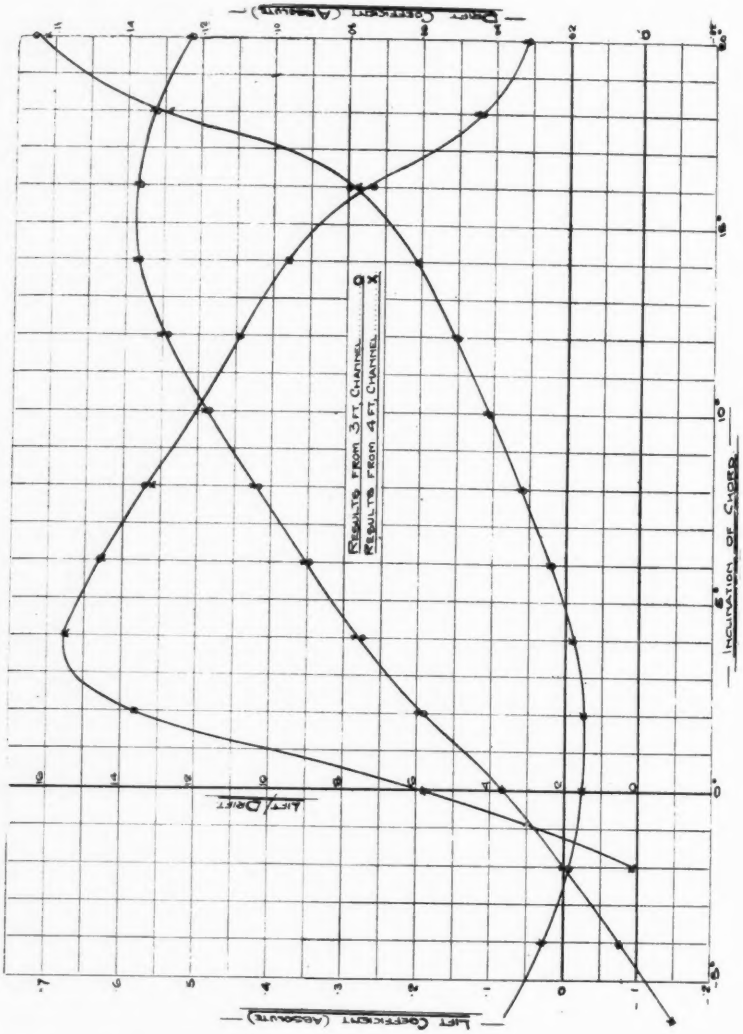

FIG. 1.

The Law of Similitude.

Next as to the means of stepping from the model to the aeroplane; it is known that the force on a surface due to the wind may be written as KSV^2, S being the area of the surface, V the speed of the wind, and K a quantity which for two similar surfaces similarly placed is approximately a constant, independent that is of the velocity and the area. If K were really constant the step from

model to aeroplane would be simple; to obtain the force on the aeroplane at a given speed it would merely be necessary to measure that on the model at some speed and increase it in the ratio of the area of surface of the aeroplane to that of the model and of the squares of the respective velocities. But experiment proves that the force is not strictly proportional to the square of the speed. If the lift and drift coefficients of an aerofoil, *i.e.*, the ratio of the lift or of the drift to the square of the speed, be determined, they are found to vary with the speed. This is shown in Figs. 2 and 3, which represent the result of such a series of experiments, and in which, as the speed changes from 10 to 50 feet per second, there is a growth in the coefficients.

At an early point in the work of the Advisory Committee for Aeronautics, Lord Rayleigh called attention to the fact that if K be not constant for similar surfaces it must depend on the quantity $\frac{VL}{\nu}$ or in mathematical terms be expressible as a function of $\frac{VL}{\nu}$ where V is the velocity of the current, L some linear dimension of the surface, and ν the kinematic viscosity of the air. If then we plot the value of K as found for an aerofoil in a given position, but for different values of the velocity against VL, the spots ought to be on a smooth curve and the form of this curve will determine K as a function of VL. This has been done in Fig. 4, where the values of the lift to the drift ratio are plotted against VL (or rather, for convenience, against log VL) for the series of experiments shown in the preceding curves.

Again, experiments have been made at the Aerodynamical Laboratory of the University of Paris on full-sized aerofoils. These have been repeated at the Laboratory on models 1-16 of the scale, and when the results are reduced by the above law, the agreement in the lift experiments is practically complete; the measurement of the drift is more difficult and the agreement is less good, but the results for the ratio are given in Fig. 4, and it appears that at the highest value of VL yet reached in the model experiments the value of the ratio lift-drift is somewhat less than for the full scale experiments, but that values for the coefficients found from the 50 ft. per sec. observations in the channel do not differ greatly from those belonging to the actual machine. This point can be checked more fully when the large channel is complete, and the necessity of checking it afforded a strong reason for the building of that channel.

There is another method, however, of checking the accuracy of the model work which has been pursued with great success. The forces on an aerofoil are the resultant of the pressures due to the wind at all points of its surface. If these pressures be measured at a sufficient number of points, the pressure distribution can be plotted and the resultant force calculated. This has been done and the results are shown in Fig. 5, where the values found by calculation are compared with those measured directly.

In the case of the lift the agreement is complete; in the case of the drift it is less good, the calculated results are generally too low, and this would be expected, for in the calculation the air friction on the surface has been neglected. This would hardly affect the lift; it would modify the drift and the ratio lift-drift considerably.

Reference may perhaps be made to another result which follows from the plotting of the pressures and to which attention has frequently been called. A very much larger proportion of the upward lift is due to the reduction of pressure on the upper side of the aerofoil than is caused by the increase of pressure on the lower side; thus, in designing a wing the shape of the upper surface is much more important than that of the lower.

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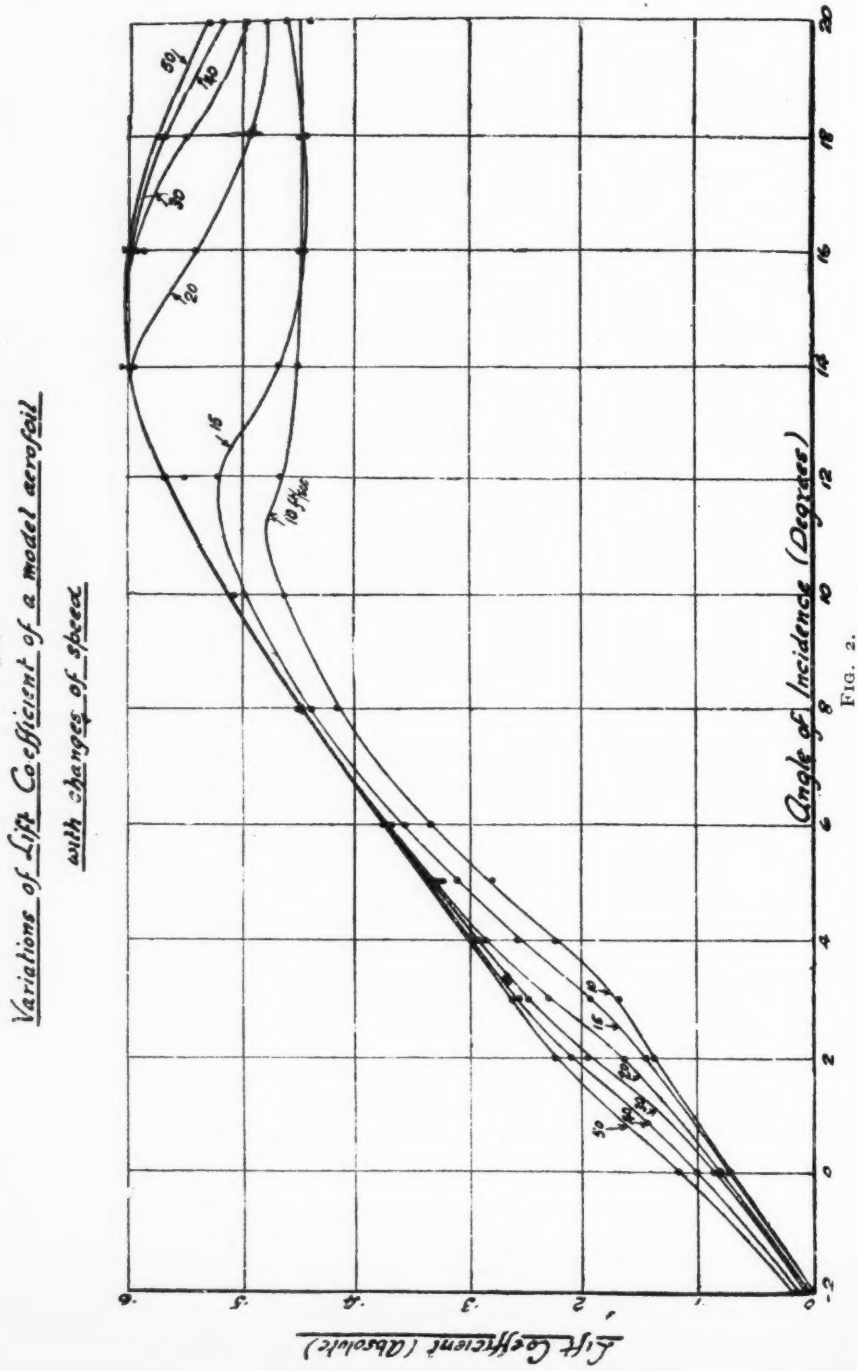
Variations of Lift Coefficient of a model aerofoil with changes of speed.
Lift Coefficient (Absolute)
Angle of Incidence (Degrees)
50
40
30
20
15
10 Miles

FIG. 2.

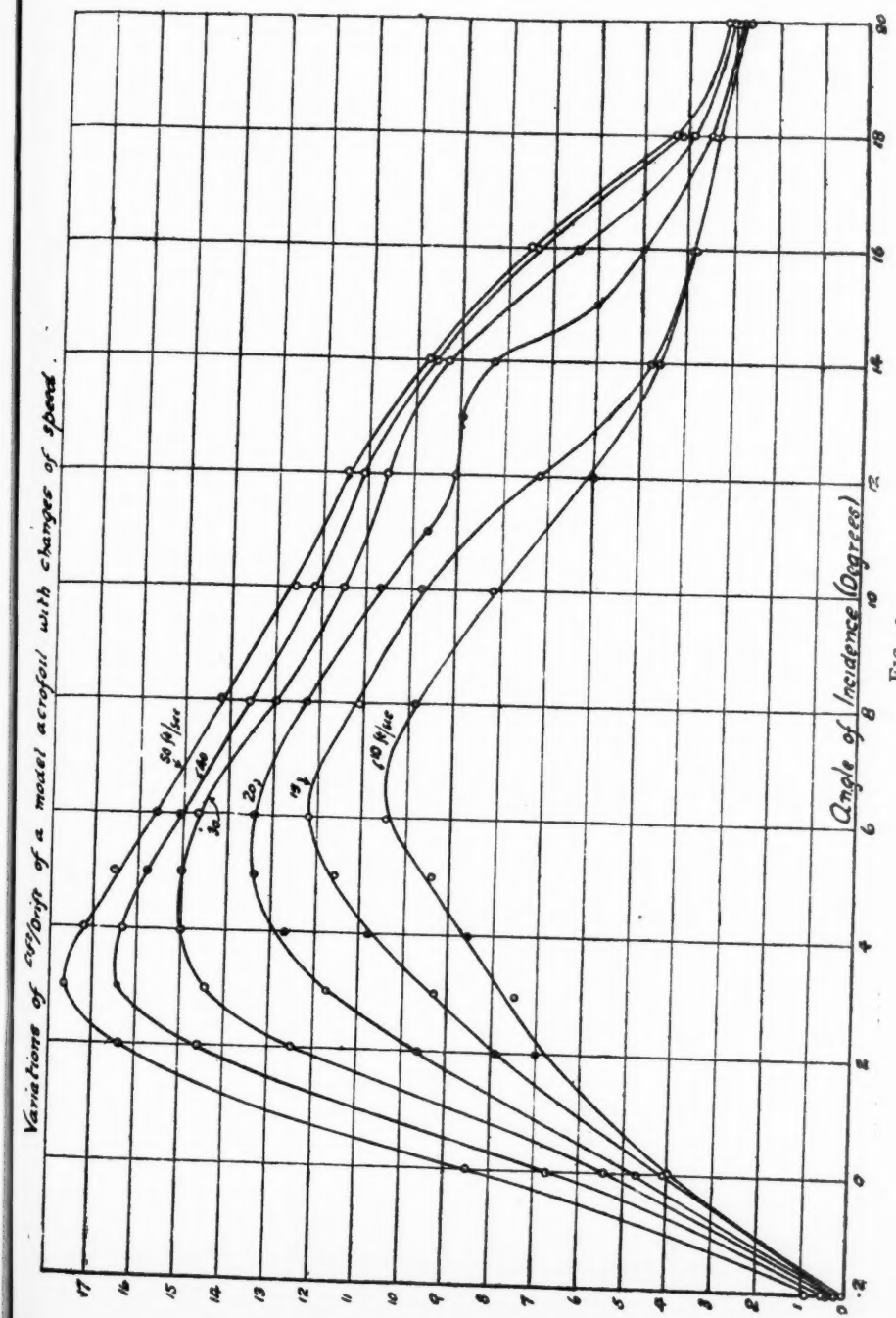
Variations of Lift & Drift of a model aerofoil with changes of speed.
Angle of Incidence (Degrees)
50 ft/sec
40
30
20
15
10 ft/sec

FIG. 3.

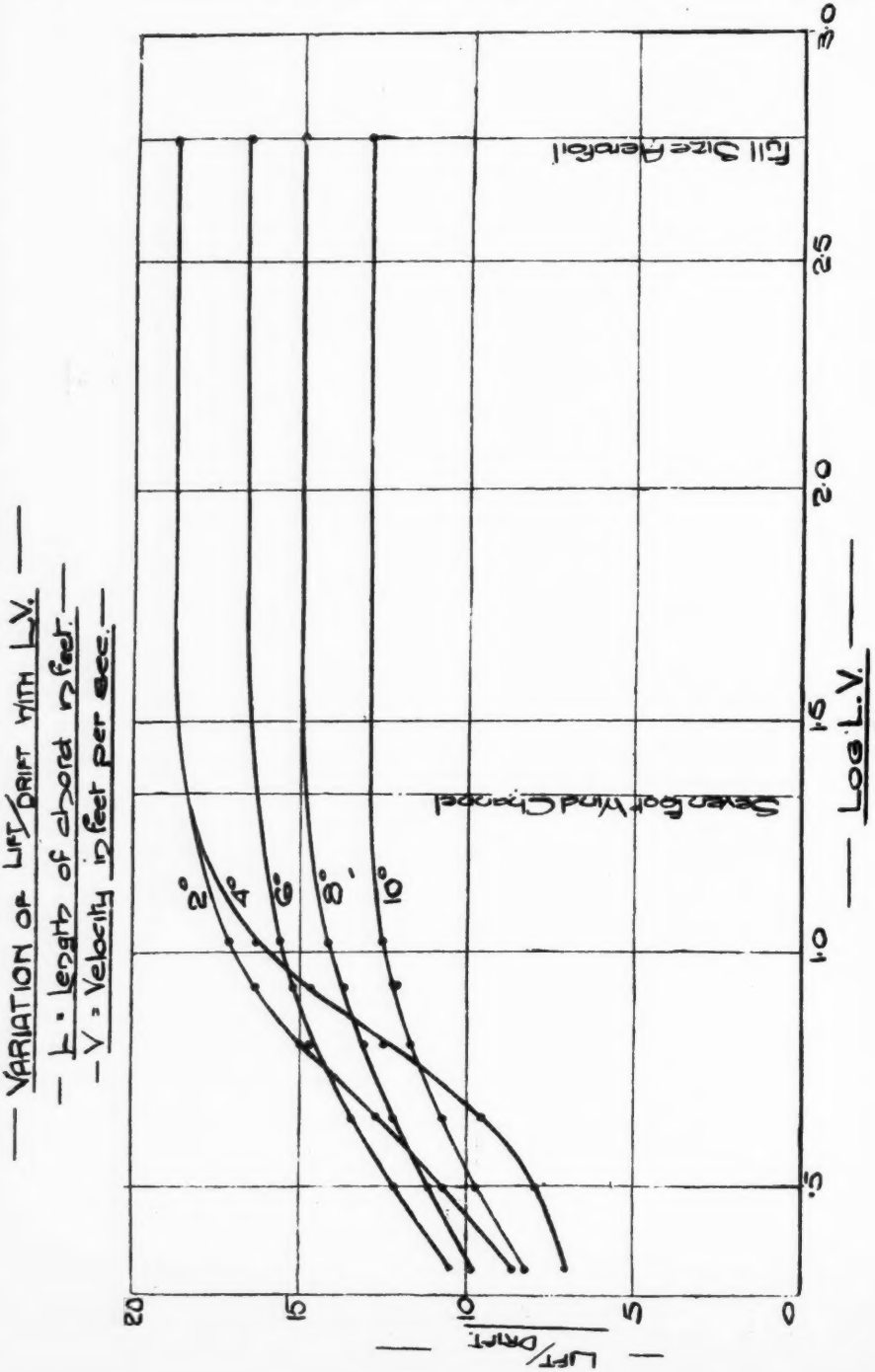
— VARIATION OF LIFT/DRIFT WITH L.V. —
— L = length of chord in feet —
— V = Velocity in feet per sec. —
Full Size Aerofoil
Seven Foot Wind Channel
2°
4°
6°
8°
10°
LIFT/DRIFT
— Log L.V. —
0
5
10
15
20
1·5
1·0
2·0
2·5
3·0

FIG. 4.

FIG. 4.

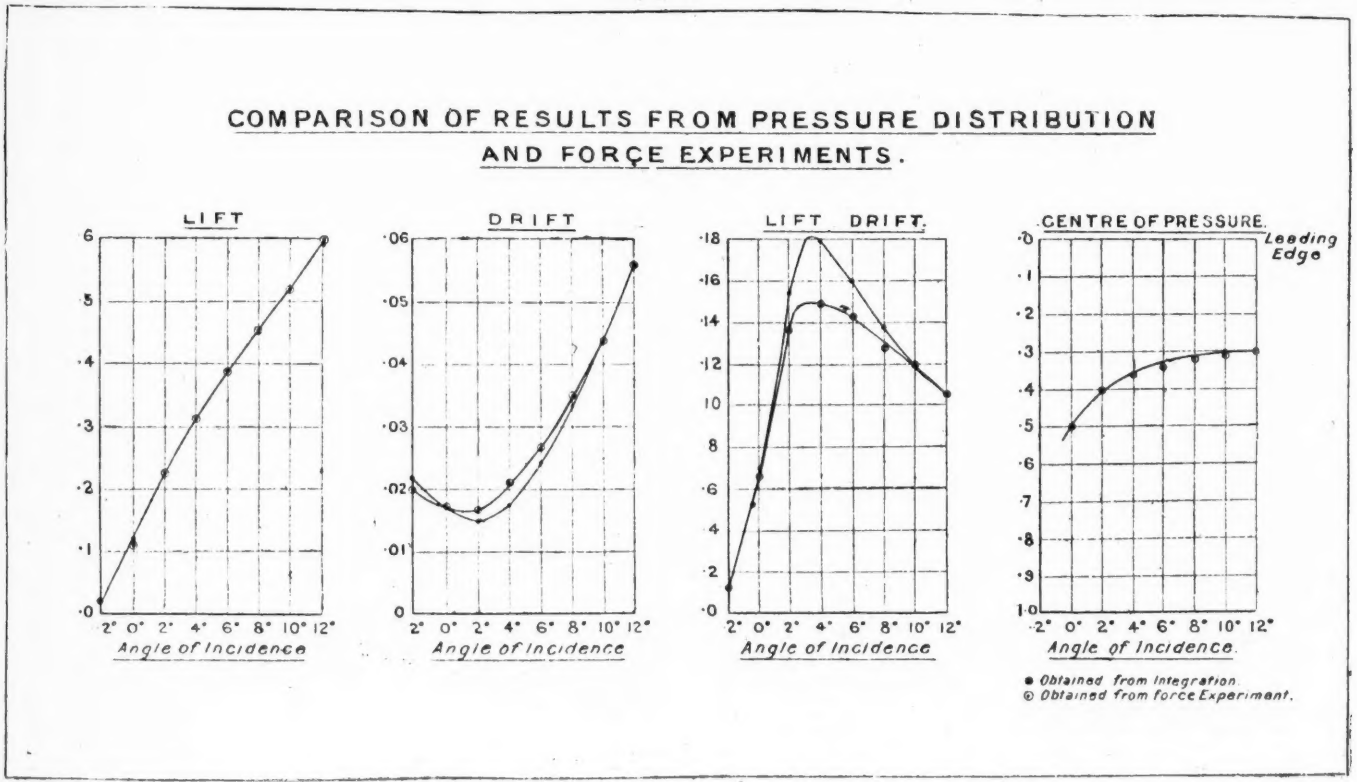
COMPARISON OF RESULTS FROM PRESSURE DISTRIBUTION AND FORCE EXPERIMENTS.
LIFT
DRIFT
LIFT DRIFT.
CENTRE OF PRESSURE.
Leading Edge
Angle of Incidence
Angle of Incidence
Angle of Incidence
Angle of Incidence.
● Obtained from Integration.
◎ Obtained from force Experiment.

FIG. 5.

The Stability of Aeroplanes.

Let me turn now to some further developments of the model experiments. The theory of the stability of an aeroplane has been developed by Lanchester, Bryan, and others, and is very complex. It depends on finding an expression for the energy of the machine in any position in terms of the velocities of its centre of gravity along the axes of reference, and of the angular velocities of the machine about these axes. This expression involves a number of constants, quantities which depend on the shape and dimensions of the machine, not on its motion. If we know the energy, values can be found for the forces and moments on the machine—these involve the same constants—and the equations of motion can then be formed. Their solutions can be obtained at any rate in certain cases of importance, but to utilise the results we require to know the numerical values of the constants just referred to and to determine these we must have recourse to the model experiments. By means of the balance the forces on the model can be measured; these forces can also be expressed, as we have seen, in terms of the constants, and the wind velocity, and hence we can find certain of the constants applicable to the aeroplane considered.

Further experiments of a somewhat different character are required to determine the values of the rest of the constants or coefficients in the energy expression —the rotary derivatives as they are called; but by means of the model experiments all these can be found, and on substituting the values in the equations of motion, the nature of the motion can in many cases be determined by the solution of the equations.

Determination of Stability Co-efficients.

The next series of figures gives some of the results of this work. A model of an ordinary type of monoplane, such as is exhibited, is supported in the channel in various positions relative to the direction of the air current and the forces measured; the axes or direction of reference are taken as shown in Fig. 6.

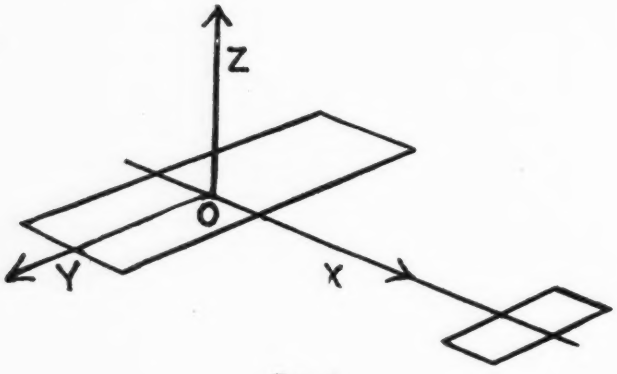

FIG. 6.

The axis of x is along the length of the machine from head to tail in the direction which is horizontal during the normal horizontal flight of the machine, and opposite to the direction of motion. The axis of z is at right angles to this in the plane of symmetry of the machine, while the axis of y is at right angles to the two, parallel, that is, to the length of the wings.

Pitching, Rolling and Yawing.

We use the term "to pitch" to mean the angular motion up and down in the plane of symmetry, turning that is about the y axis; "to roll" or bank, to mean rotation about the length of the machine, that is about the x axis, and "to yaw" to turn to right or left about the axis of z. The angle of pitch is positive when the nose of the machine rises, the angle of yaw is positive when the machine turns to the right, and the angle of banking which properly accompanies this turn to the right will also be positive.

Fig. 7 gives the forces and moments which are produced in the plane of symmetry when the attitude of the machine to the wind changes, but without yawing, while in Fig. 8 are shown the forces and moments produced by yawing without altering the angle of pitch, so that the flight is horizontal. The wind speed for which the forces are given is 30 ft./sec.

Starting from zero pitch angle the longitudinal force falls as the angle of attack is increased, reaches a minimum at about 8°, and then rises again rapidly. The normal force increases regularly as the angle of attack increases, while the pitching moment increases in amount but is negative, that is to say, it tends to reduce the angle of attack.

The machine is stable longitudinally, so far as pitching moment on it is concerned, but further investigation is required before the motion can be completely determined.

Turning now to Fig. 8 we see that as the angle of yaw increases the longitudinal and normal forces are somewhat reduced though the changes are not at first large, but a considerable negative lateral force is brought into action, if the machine turns to the right the side force is from left to right, the machine side-slips in the direction in which it is turned. There is a yawing couple N which is negative, *i.e.*, tends to reduce the angle of yaw and turn the nose of the machine into the wind, at the same time a positive rolling moment L is produced, the machine tends to bank as required for the turn, and also at first we have a positive pitching moment M, the angle of attack is increased, and the nose of the machine rises.

The next figure, Fig. 9, gives the curves of pitching moment of a biplane model for various settings of the elevator in the tail, the wind speed in this case being 40 ft./sec. Without the tail plane the angle of attack for horizontal flight would be slightly negative, but the machine in this state would be unstable, any increase or decrease in the angle of attack causes a moment tending still further to increase or decrease the angle respectively and so to disturb the machine. With the tail plane the machine flies though with no great longitudinal stability when the elevator is not raised. The effect of raising or lowering the elevator is shown in the curves. Positive angles correspond to a depression of the elevator tending to lower the nose of the machine.

The Wash from the Main Planes affects the Tail.

As the result of experiments of this kind it appears that the effect of the tail on the longitudinal balance is much less than would be anticipated if it is supposed that the air current is not deflected by the planes of the machine and is free to act on the tail as though the main planes were absent. The wash from the main planes reduces the moment on the tail very greatly. This is shown in Fig. 10, which gives in the upper curve the moment about the C.G. of the machine as calculated from a knowledge of the shape and position of the tail, the elevators being at a small positive angle. The lower curve gives the measured moment which is of about only half the amount.

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VARIATION OF FORCES & MOMENTS ON MODEL BLERIOT TYPE MONOPLANE WITH ANGLE OF PITCH.

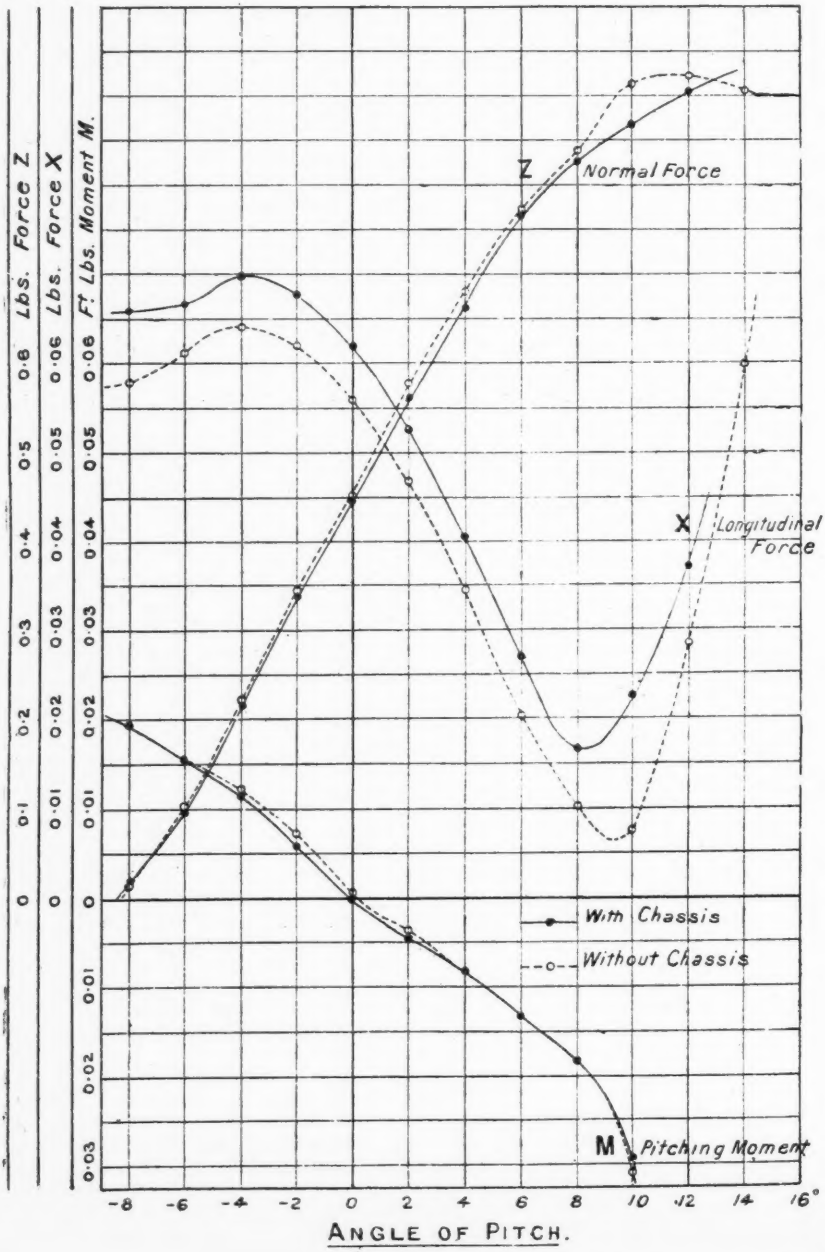

Lbs. Force Z
0 0·1 0·2 0·3 0·4 0·5 0·6
Lbs. Force X
0 0·01 0·02 0·03 0·04 0·05 0·06
Ft. Lbs. Moment M.
0·06 0·05 0·04 0·03 0·02 0·01 0 0·01 0·02 0·03
Z Normal Force
X Longitudinal Force
With Chassis
Without Chassis
M Pitching Moment
-8 -6 -4 -2 0 2 4 6 8 10 12 14 16°
ANGLE OF PITCH.

FIG. 7.

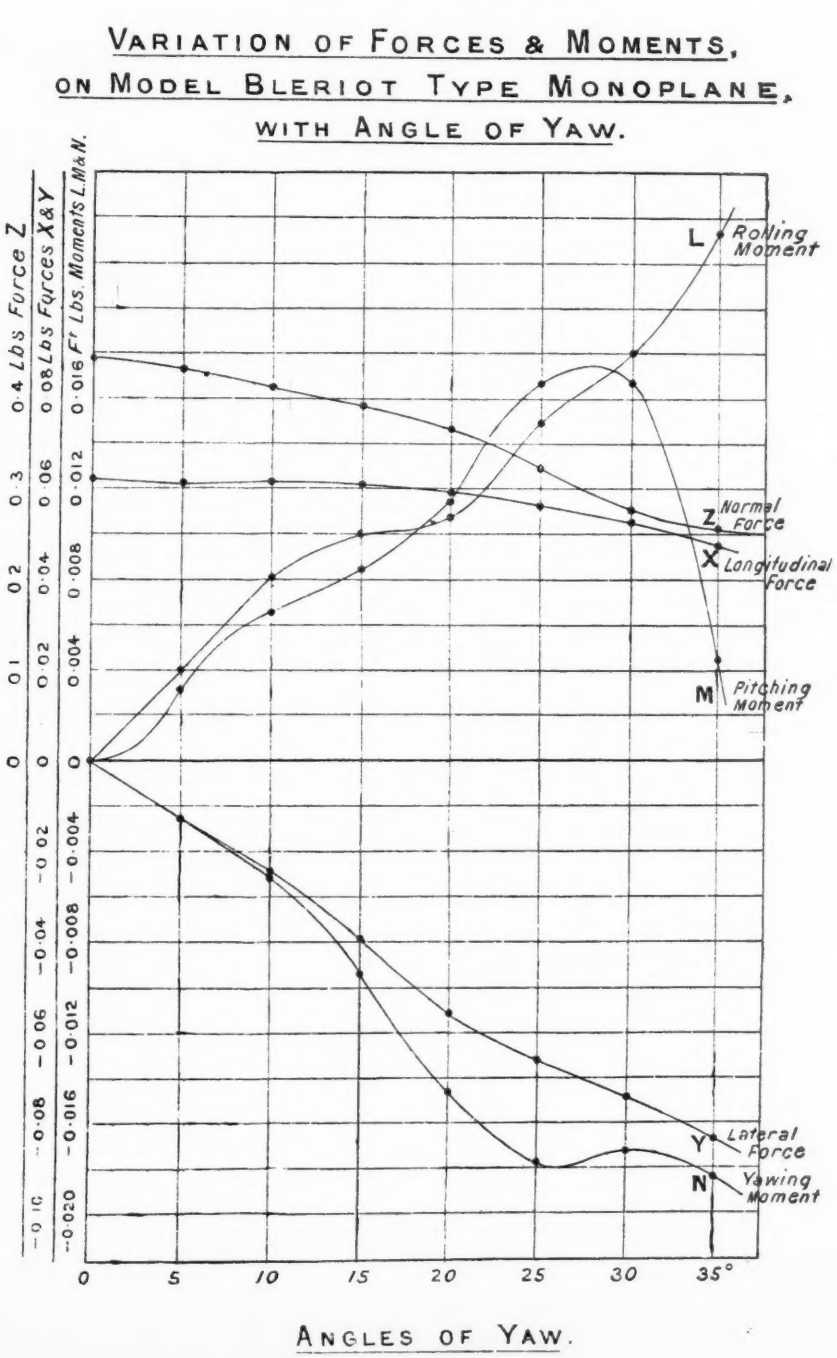

VARIATION OF FORCES & MOMENTS,
ON MODEL BLERIOT TYPE MONOPLANE,
WITH ANGLE OF YAW.
0·4 Lbs Force Z
0·08 Lbs Forces X & Y
0·016 Ft Lbs. Moments L.M & N.
L Rolling Moment
Z Normal Force
X Longitudinal Force
M Pitching Moment
Y Lateral Force
N Yawing Moment
ANGLES OF YAW.

FIG. 8.

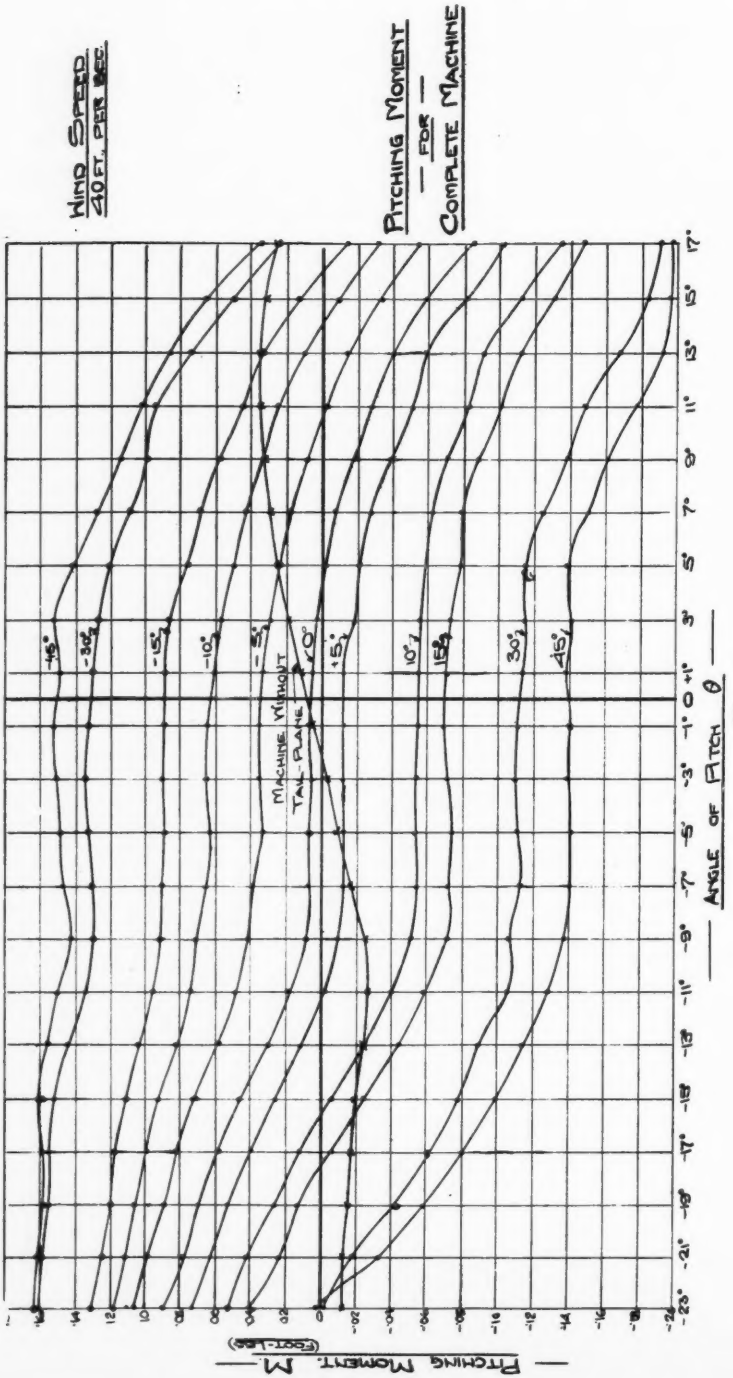

Wind Speed
40 ft. per sec.
Pitching Moment
— for —
Complete Machine
Machine without tail plane
+15°
+10°
+5°
0°
-5°
-10°
-15°
-30°
-45°
15°
30°
45°
— Pitching Moment M —
(foot-lbs)
— Angle of Pitch θ —

FIG. 9.

To account for this, observations were made of the distribution of velocity round the tail, and the middle curve—marked calculated effective tail—was obtained from these. The difficulty of the measurement accounts probably for the outstanding difference, but the matter is still under investigation. As a result, we find that an alteration of the inclination of the machine to the wind produces an alteration of only about half the amount in the direction of the wash from the planes, and thus causes much less change in the moment due to the

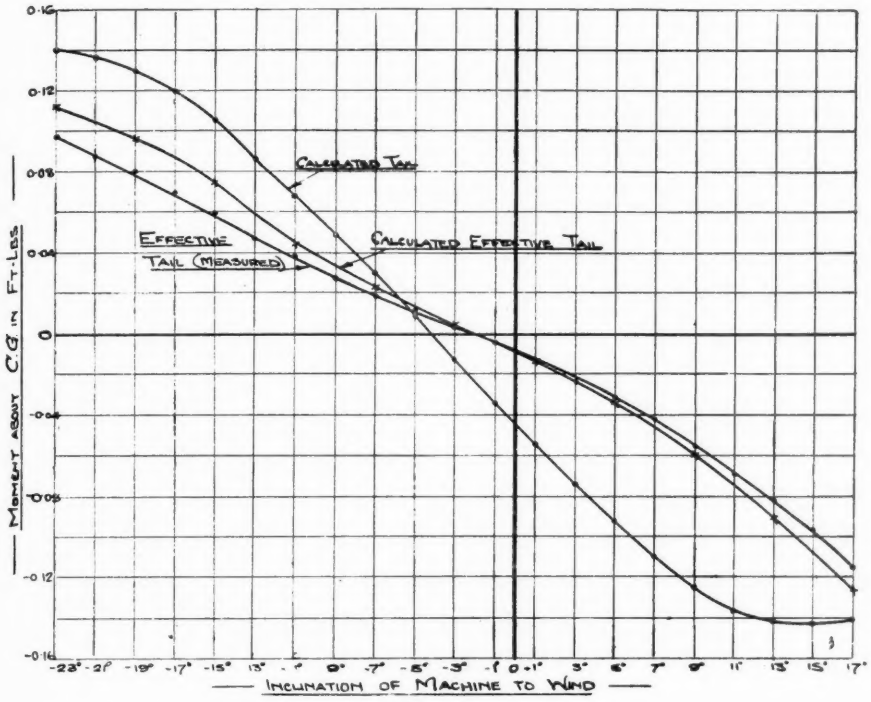


FIG. 10.

tail than would be anticipated from the change in attitude of the machine. The matter is of importance and requires further study to determine the best position for the tail.

So far we have been supposing the aeroplane to be in steady flight in a uniform wind. Let me remind you what the mathematics show will happen if it be disturbed from this condition.

Disturbed Flight in a Non-uniform Wind.

In the first place we may deal separately with the motion of the machine in the vertical plane of symmetry, the longitudinal motion as it is called, and its lateral motion or motion perpendicular to that plane. If the longitudinal motion be slightly disturbed and the machine be stable, two kinds of oscillations are set up; one kind consists of rapid oscillations, both of the machine about its centre of gravity and of the centre of gravity itself. Ordinarily, in a stable machine, these are quickly damped down and give no trouble. But besides these the centre of gravity of the machine itself describes an undulating path; the

period of these undulations is much longer than in the first series, though for a stable machine the amplitude of the oscillation is gradually reduced and the machine, if no other changes have been made, gradually recovers its normal flight path. Mr. Lanchester has called these slower oscillations the phugoid oscillations.

If the machine is not stable, when disturbed from its normal path, its motion will diverge more and more from the condition of steady flight, and this divergence may take place continuously or it may happen that a series of oscillations of increasing amplitude is set up which ultimately become so large as to cause disaster. It is not possible to predict exactly what will happen in a given case without a knowledge of the relative values of the constants or coefficients already referred to, but if these be known the equations can be solved numerically and the details of the flight can be predicted.

We can go further than this, as Mr. Bairstow has shown in the beautiful model experiments he carried out recently before the Society, and determine the alterations which must be made in the machine to produce definite modifications in the conditions of its flight.

Now we have already seen in Figs. 7 and 8 the curves of forces and moments from which the constants can be determined for the model monoplane which has been exhibited. Mr. Bairstow and Mr. Nayler have recently determined the coefficients for this machine and used it to determine its motion under a variety of circumstances, and I propose to give some account of their results.

The Effect of a Single Horizontal Gust in the Direction of Motion.

Let us suppose the machine is flying horizontally against a uniform wind and at a certain moment the horizontal velocity of the wind changes by a known amount, u_0 say, and remains permanently at its new value. What are the consequences to the machine? You may say at once, what is the use of doing this? It is rarely that an airman finds a perfectly steady wind in which to fly; it is practically impossible that that wind should change quite suddenly and remain steady with the new velocity. Real gusts are much more complex, why trouble about a purely hypothetical case? Well, but we must go slowly; we must walk before we run; step by step, line by line, here a little, there a little, is the way we must advance, and I shall hope to show you ere I close, how from the solution of some simple hypothetical cases we can obtain information as to what happens in actual flight.

Fig. 11 gives the solution of our problem; the longitudinal motion only is affected and the results on the components of this motion are shown in the curves. The quantities we have to deal with are u, the change in the horizontal velocity relative to the surrounding air; w the normal velocity, θ the pitch angle or angle of attack, and q the angular velocity of the machine in the plane of symmetry; q is of course equal to $\frac{d\theta}{dt}$

At a certain moment the velocity of the wind changes by an amount u_0. At that moment, therefore, the velocity of the aeroplane relative to the air is increased by an amount u_0, but this increase rapidly dies away, and after five seconds has become zero; the velocity goes on decreasing for five seconds more, and at the end of some ten seconds has reached its minimum value, which is less than the original steady value by about $\frac{1}{2}u_0$. The velocity then increases again for some ten or eleven seconds and the changes follow a regular periodic curve of rapidly decreasing amplitude, and after some 40 or 50 seconds are completely damped out. Dealing next with w, the normal velocity, we find it starts from

zero, but rises rapidly in a fraction of a second to its maximum amount, about 2-10 of u, and then dies away rapidly in the same manner as u.

The angular velocity q is small, so small that the curve is drawn to give 100 times q, and this follows practically the same law as w, while θ, the pitch angle, increases for five seconds and then decreases to pass through period changes of decreasing amplitude and of about 22 seconds period.

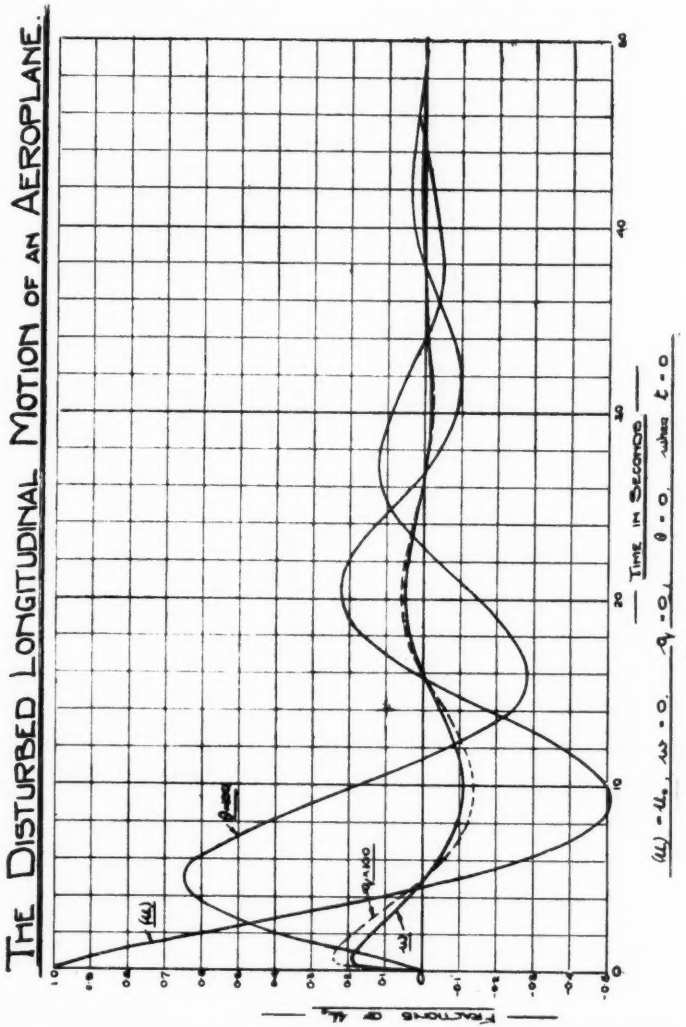

THE DISTURBED LONGITUDINAL MOTION OF AN AEROPLANE.
FRACTIONS OF u_0
TIME IN SECONDS
$(u) = u_0$, $w = 0$, $q = 0$, $\theta = 0$, when $t = 0$
$q \times 100$
θ

FIG. 11.

The motion is stable; the complete calculations show that the rapid oscillations in this case only affect w and q, and that they die out in less than a second, while the other disturbances are those arising from the phugoid motion of the machine. Thus, in this case the machine when struck by the horizontal gust loses longitudinal speed at first and after passing through a series of changes of velocity, settles down after a few oscillations in less than a minute to its original speed

relative to the wind. This loss of speed is accompanied by an initial increase of normal velocity; the machine rises for a fraction of a second, acquiring a rapid positive angular velocity, but these motions soon change sign and die away like the horizontal velocity. The nose of the machine rises for five seconds, at first rapidly, then more slowly, and this oscillation dies down in the same manner as the others.

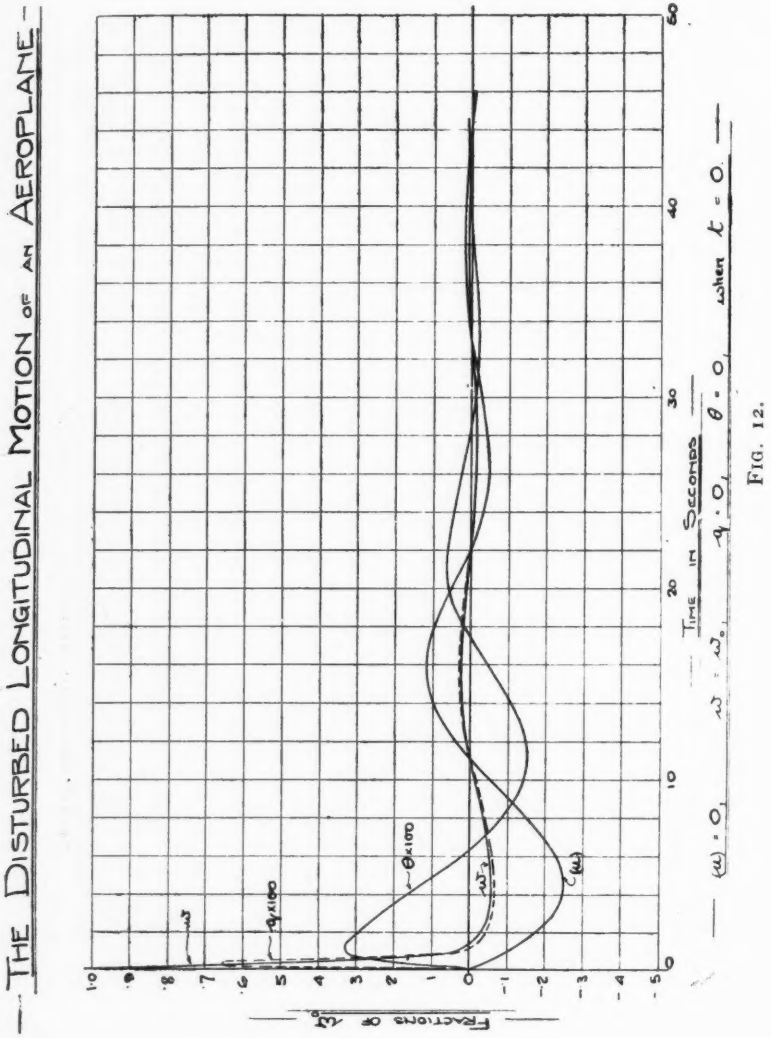

THE DISTURBED LONGITUDINAL MOTION OF AN AEROPLANE
TIME IN SECONDS
FRACTIONS OF $\frac{V}{V_0}$
$\theta \times 100$
$q \times 100$
w_3
(u)
$(u) = 0$, $w = w_0$, $q = 0$, $\theta = 0$, when $t = 0$

FIG. 12.

A Single Downward Gust in the Plane of Symmetry.

The next figure, Fig. 12, gives the changes due to a downward gust w. Relatively to the air the machine acquires an upward velocity w_0, which dies down in about one second and is followed by the slow phugoid changes as before.

The changes in the other quantities are shown in the curves and the motion of the machine can be traced as before.

By combining the results of these two diagrams we can find the effect of a steady gust striking the machine in any direction in the plane of symmetry. In a similar way we can find the effect of a change in the direction of the wind or of an alteration in the propeller thrust or of the position of the elevators. The last is shown in Fig. 13, in which it is supposed that the elevator is turned so as to cause the machine to climb.

Lateral Disturbance.

The next two figures indicate what the motion will be in two cases of lateral disturbance. This motion, in the particular machine dealt with, is unstable. Its expression involves three terms. One of these corresponds to a periodic oscillation rather heavily damped, having for one machine a period of about six seconds; the second to a considerable disturbance in certain of the co-ordinates, which however subsides or dies out very rapidly; and the third to a gradually increasing disturbance which gets larger with the time, according to an exponential law. The subsidence depends on terms such as $Ae^{-\lambda_1 t}$ and the divergence on terms involving $Be^{+\lambda_2 t}$, where λ_1, λ_2, are roots of a certain equation.

Side Gusts.

In Fig. 14 is shown the effect of a side wind v_0 striking the machine on the left-hand side. The curves show chiefly the effect of the slow oscillation, though the rapid increase in the value of p, the velocity of rolling, is due to the subsidence term. The machine quickly picks up the velocity of the wind; after some seven seconds v, the relative sideways motion, is very small, but it gradually increases, and after about 40 seconds has reached some 9% of the original disturbance. Unless the controls are altered the side-slip will continue to increase. A large angular velocity of roll p is started almost immediately, and at first this gradually dies down, but after six seconds or so the divergence term begins to tell and the rolling continues to increase unless checked by the pilot.

Again, r the velocity of yaw is at first negative, the machine yaws to the left, a motion opposite to that which corresponds to the bank; after a time this is reversed and yaw and bank increase together.

Sudden "Banking."

In Fig. 15 we have recorded the effect of sudden banking through an angle ϕ_0. The effects of the subsidence and oscillation terms are not large, but those of the divergence terms are very great; after 40 seconds the angle of banking exceeds its original value by 63%, while r, the velocity of yaw, also increases rapidly, as does v the side-slip velocity, which takes place in the negative direction. Thus, the machine turns to the right, increasing the angle of banking and side-slipping inwards and downwards at the same time.

In all the descriptions above it has been assumed that the controls are not touched, but a comparison of Fig. 15 with the curve obtained when the effect of warping or of turning the rudder are considered show that the control of such a machine is not easy.

Thus, Messrs. Bairstow and Nayler have solved the following problems:—An aeroplane is in flight in the air—(1) At a given instant the wind changes either in speed or direction or both, and the new conditions remain for a time steady. The motion of the aeroplane is determined by the curves given in the

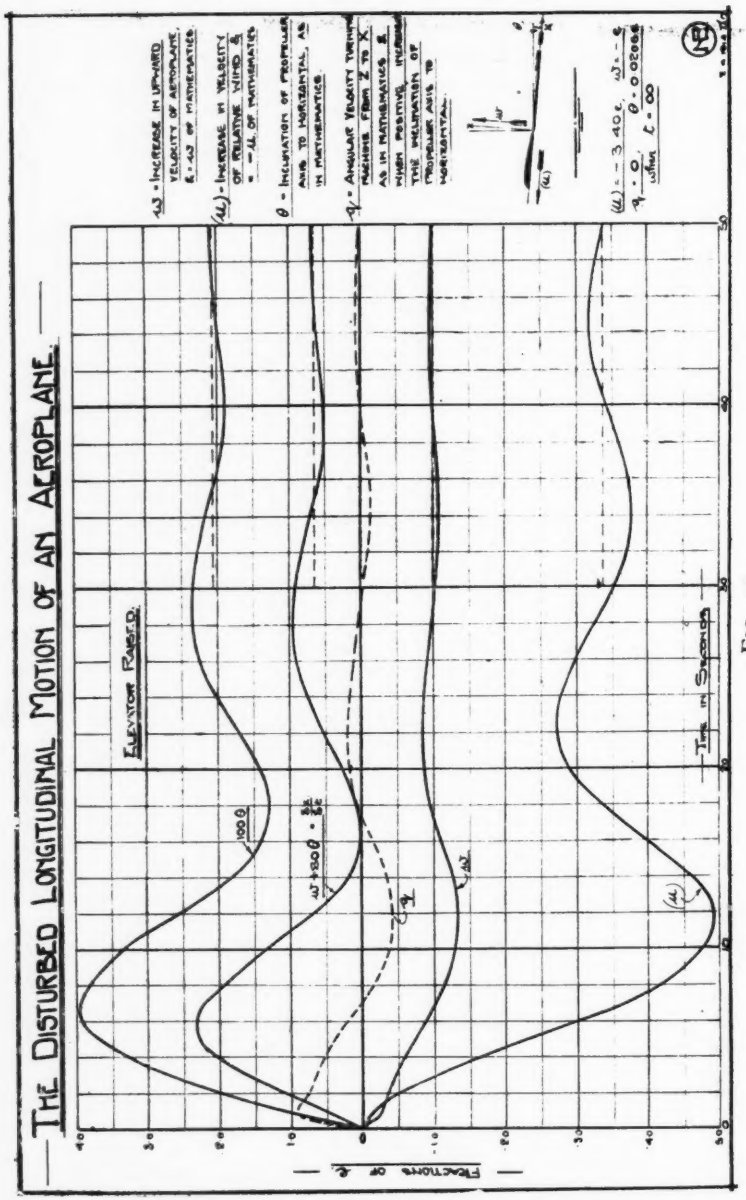

THE DISTURBED LONGITUDINAL MOTION OF AN AEROPLANE.
ELEVATOR FIXED.
u - INCREASE IN UPWARD VELOCITY OF AEROPLANE, E.G. OF MATHEMATICS.
(du) - INCREASE IN VELOCITY OF RELATIVE WIND E.G. = -du OF MATHEMATICS
θ - INCLINATION OF PROPELLER AXIS TO HORIZONTAL AS IN MATHEMATICS
q - ANGULAR VELOCITY TURNING AS IN MATHEMATICS FROM Z TO X
HIGH POSITIVE INCIDENCE
THE INCLINATION OF PROPELLER AXIS TO HORIZONTAL
(u) = -3.40 ft. (u)=-5
q = 0 θ = 0.02664
when t = 00
TIME IN SECONDS
FRACTIONS OF t
100 θ
u'+100 θ - ∂x/∂t
q
(u)
(du)

FIG. 13.

paper; (2) at a given instant the controls of the aeroplane are altered. The ensuing motion is defined by other curves; and (3) by a suitable combination of the curves the effect of change of wind and change of control occurring simultaneously can be determined.

The Effect of Successive Gusts.

They have however gone much further than this. The wind does not change instantaneously and remain steady. Let us investigate the effect of supposing that at the end of each consecutive second there is a change, but that during

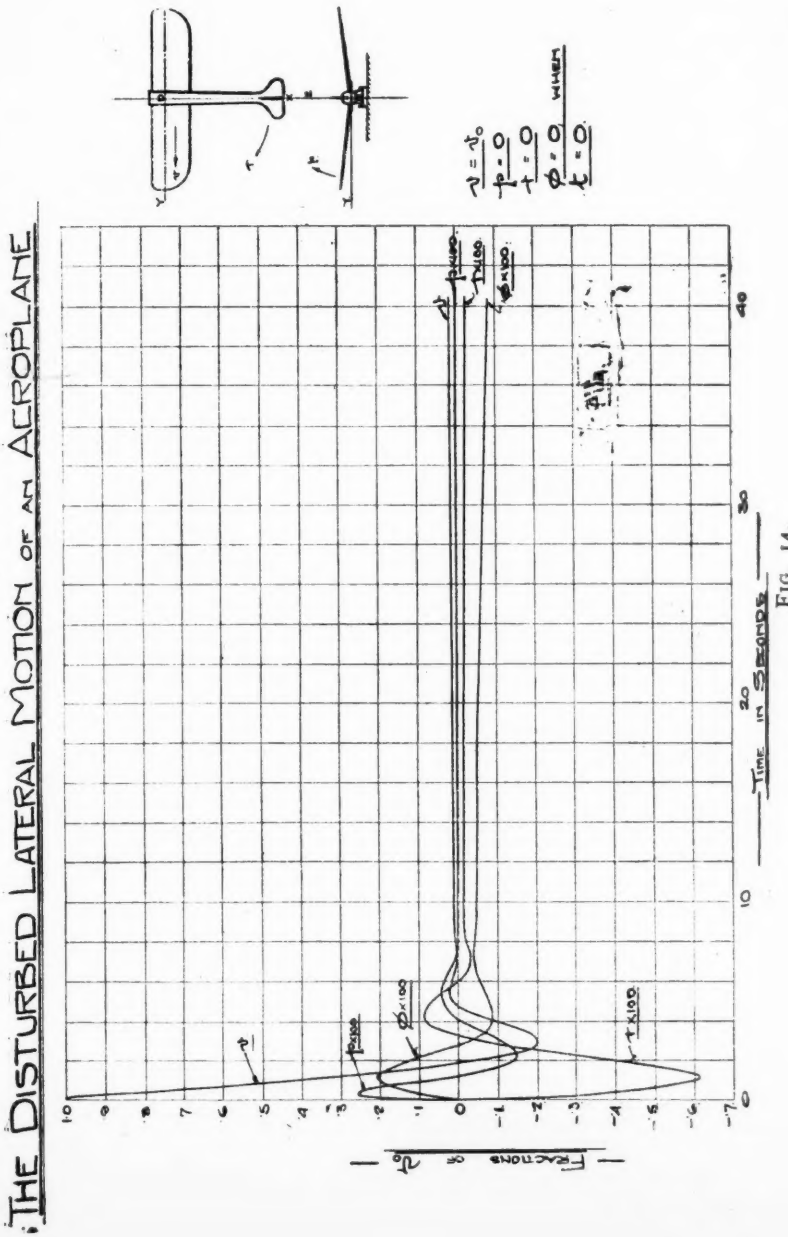
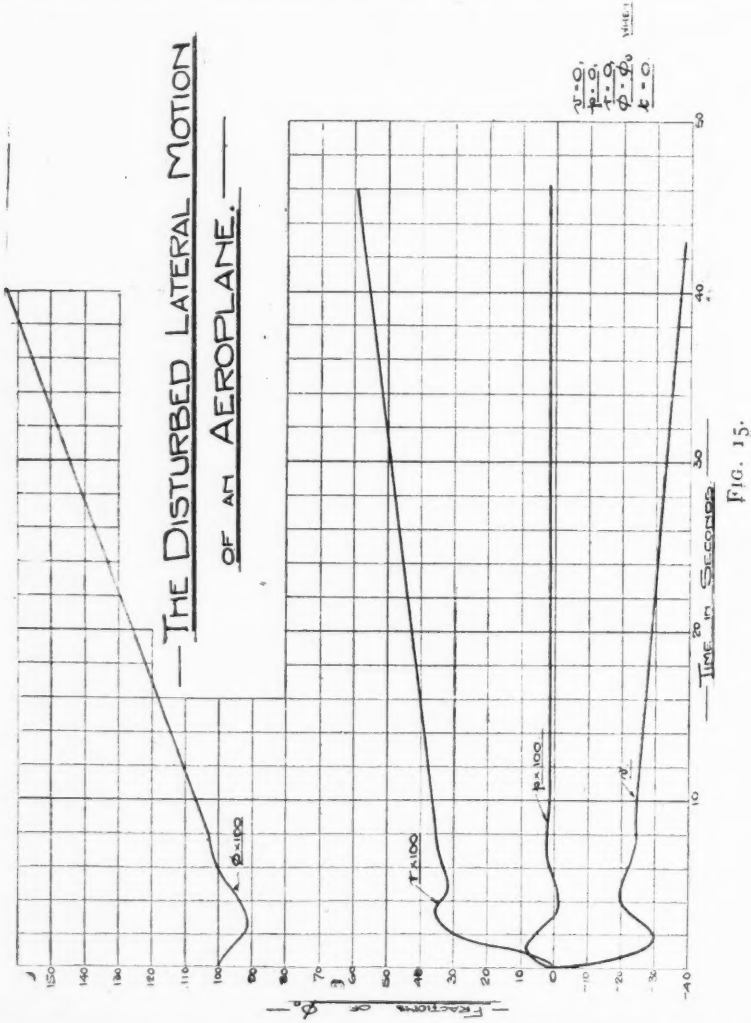


FIG. 14.

each second the wind is steady. Clearly this is a closer representation of the facts, and if we go further and take as our divisions of time, not seconds, but small fractions of a second, we get a proper representation.

We start with a knowledge of the motion of the aeroplane and the air at the commencement. Certain changes occur suddenly, and then for one second the atmospheric conditions remain steady. We can determine the motion of the aeroplane at the end of that second; then further atmospheric changes occur, treating the commencement of the second second as our origin of time, we know the motion



of the aeroplane at that instant and our curves applied again will give it at the end of the second second, and we can proceed thus for each consecutive second and so find the resulting motion.

The operation is, of course, a very tedious one if the motion has to be

investigated over any long period of time; its value consists in the fact that we can thus predict what will be the motion of an aeroplane having a definite amount of stability when moving in gusty air, if we analyse it thus for a few minutes, and we can thus determine whether either the safety of the machine or the comfort of the passenger require a modification of the stability.

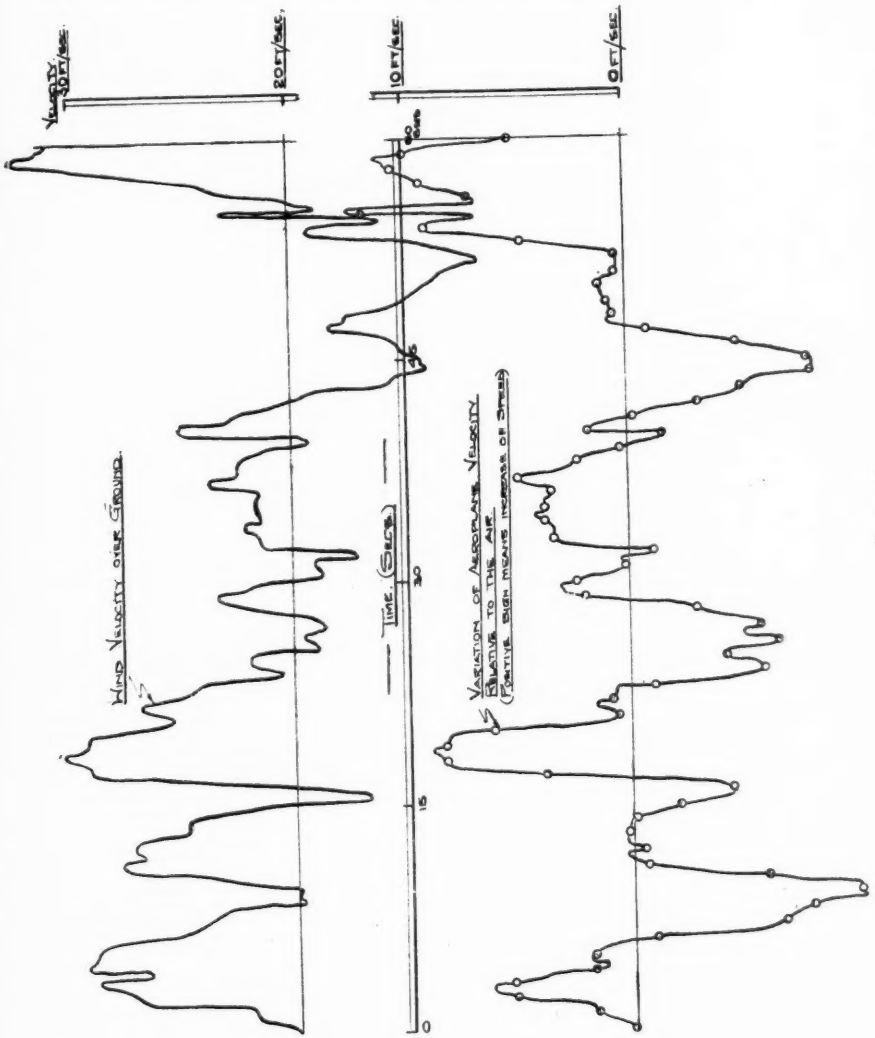


FIG. 16.

The Motion of an Aeroplane in a Gusty Wind.

Now, for some experiments of Dr. Stanton, the variations of the horizontal velocity of the wind at the Kew Observatory were taken on a quick running anemograph and thus an open scale record of the velocity changes was obtained. The velocity ranged from 11 ft. to 33 ft. per second, the average being 20 ft.

per second, and an aeroplane moving with steady speed over the ground of, say, 60 ft./sec. in the 20 ft./sec. wind, meets this gust. Messrs. Bairstow and Nayler have analysed this motion for a complete minute, assuming that the velocity of the wind changes continuously.

Figs. 16, 17 and 18 give the result. In Fig. 16 the upper curve shows the changes in the wind velocity during the minute. Starting at 20 ft./sec. it rises suddenly for some four or five seconds, then there is a sudden drop, followed by an equally sudden rise, to drop again after some eight or nine seconds to the original 20 ft./sec., and so on through the minute. The lower curve gives the variation in the velocity of the air relative to the machine during the same minute; the similarity of the two curves is marked.

The changes in air velocity relative to the aeroplane are much the same as those relative to the ground. The inertia of the machine and its small resistance prevent it from taking up the rapid changes in the air motion, and it feels the gusts much as though it were fixed. If we take the speed of the aeroplane over the ground in still air as 80 ft./sec. its speed relative to the gusty air varies from 70 to 94 ft./sec., while the speed of the air varies from 11 to 33 ft./sec. The aeroplane has not time to respond to the rapid changes. The attitude of the aeroplane to the wind alters during the time considered, it acquires a normal velocity at right angles to its own axis. The variations of this and of the vertical velocity relative to the ground are shown in Fig. 17. The top curve represents, as before, the changes in the wind velocity.

In Fig. 18 are shown the variations of the aeroplane velocity over the ground obtained by combining the two curves of Fig. 16, giving the variations of the velocity of the machine relative to the air and of the air relative to the ground, and also the variation in height of the aeroplane obtained from the vertical velocity curve of Fig. 17. While the changes in the actual horizontal velocity of the aeroplane are considerable they occur much more slowly than in the wind velocity curve, the minor alterations are wiped out; a rise in the wind velocity causes a fall in the velocity of the machine, provided the changes are sufficiently prolonged, but a very rapid rise and fall of the wind velocity is hardly noticeable. This aeroplane would take care of itself probably without undue discomfort to the pilot and without any manipulation of the controls in a gust such as is indicated, provided at least it had height in which to do it, but the changes of height involved are fairly considerable and amount, as will be seen from the figure, to rather over 60 feet in the course of the minute.

It is assumed, of course, that the controls have not been touched while this motion is in progress. In Figs 19 and 20 the effect of altering the elevator during the gust is shown, and it appears that the elevator can, without difficulty, be so manipulated as to cancel the effect of the gust.

The curves shown, of course, deal only with the longitudinal motion of the machine. I may claim, I think, that they constitute a distinct advance in our knowledge of what happens to an aeroplane in flight and may congratulate Mr. Bairstow and Mr. Nayler on the success of their calculations. They are now engaged in the similar problem for the lateral motion of the machine, and when that is completed propose to attack in the same way the motion of a biplane of standard form.

And now you may ask—What is the practical outcome to airmen of all this theory? How are they to utilise the theory, the curves and the calculations?

The machine, R.E. 1, to which Colonel Seely referred last night, gives the answer. The design of that machine at the Royal Aircraft Factory is the consequence of the experiments I have been describing to you. It is because Mr. O'Gorman and Mr. Busk and the other workers at Farnborough have known how to ask for experimental work which would help them, and how to utilise in practical design the results the Laboratory supplied to them, that it has been

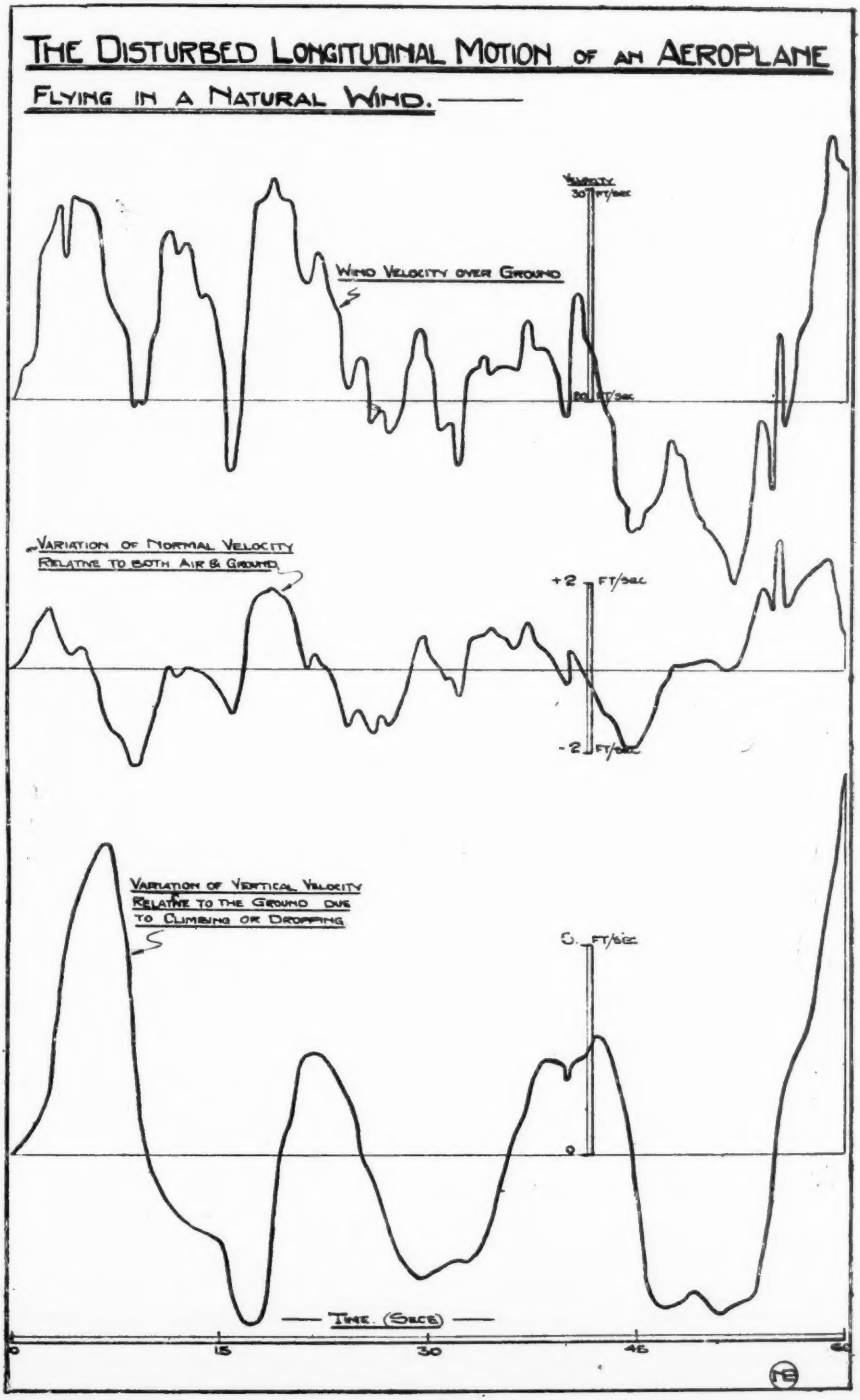

THE DISTURBED LONGITUDINAL MOTION OF AN AEROPLANE
FLYING IN A NATURAL WIND.
VELOCITY
30 FT/SEC
WIND VELOCITY OVER GROUND
VARIATION OF NORMAL VELOCITY
RELATIVE TO BOTH AIR & GROUND
+2 FT/SEC
-2 FT/SEC
VARIATION OF VERTICAL VELOCITY
RELATIVE TO THE GROUND DUE
TO CLIMBING OR DROPPING
5 FT/SEC
0
TIME (SECS)
0
15
30
45
60

FIG. 17.

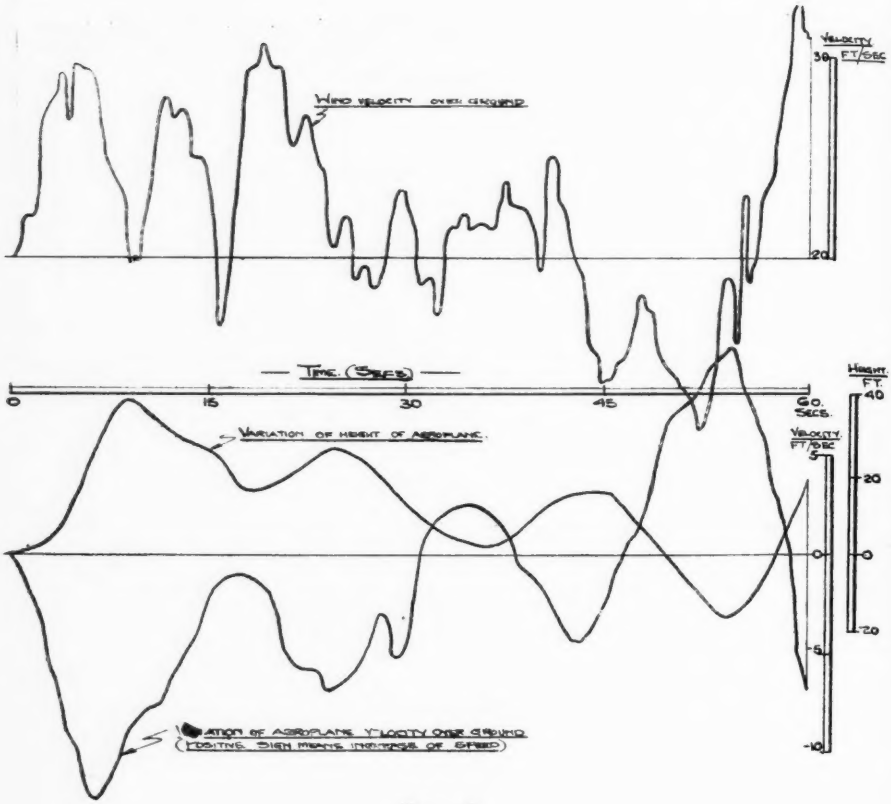


FIG. 18.

possible to design an inherently stable machine and to show when the machine is built that it will do all that its designers had in mind.

R.E. 1 marks an epoch in construction, not because it is the first machine which has flown for 15 minutes without touching the controls. That honour belongs, it is possible, to some other machine, but because it is the first machine in which the exact balancing of its various parts has been so calculated as the result of experimental work that it shall be inherently stable, and because in actual flight it has shown that those calculations have been verified.

I am glad that it falls to me in this second Wilbur Wright lecture to note the advent of such a machine and congratulate and thank the officials of the Royal Aircraft Factory for the advance they have achieved.

The Strength of Aeroplanes.

There is another matter besides stability on which recent investigations throw considerable light. What are the stresses to which a machine may be subject in flight? How strong ought it to be made? What is a proper factor of safety to adopt? The matter has, during the past six months, received much attention from the Advisory Committee for Aeronautics. Let me briefly state some of the more important results, and first, what do we mean by the term factor of safety?

THE LONGITUDINAL MOTION OF A CONTROLLED AEROPLANE IN A WIND

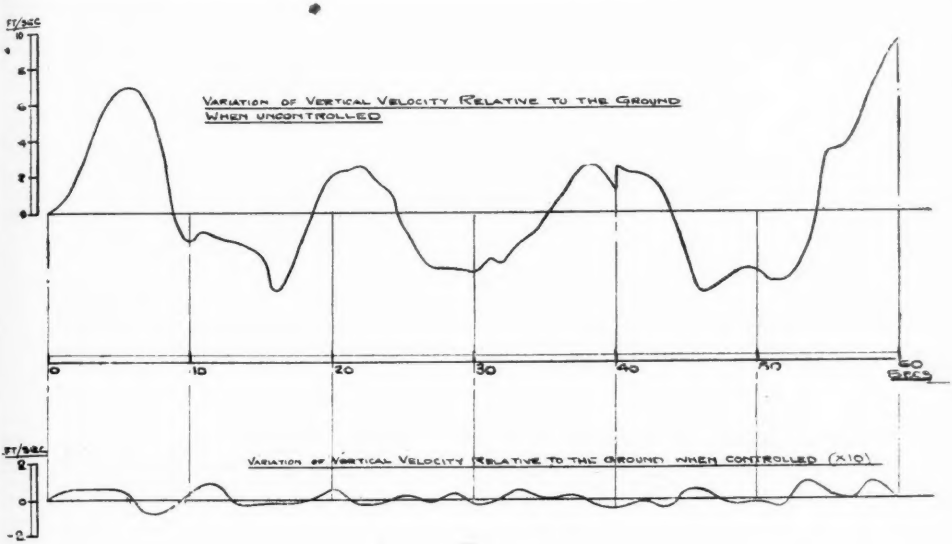


FIG. 19.

THE LONGITUDINAL MOTION OF A CONTROLLED AEROPLANE IN A WIND

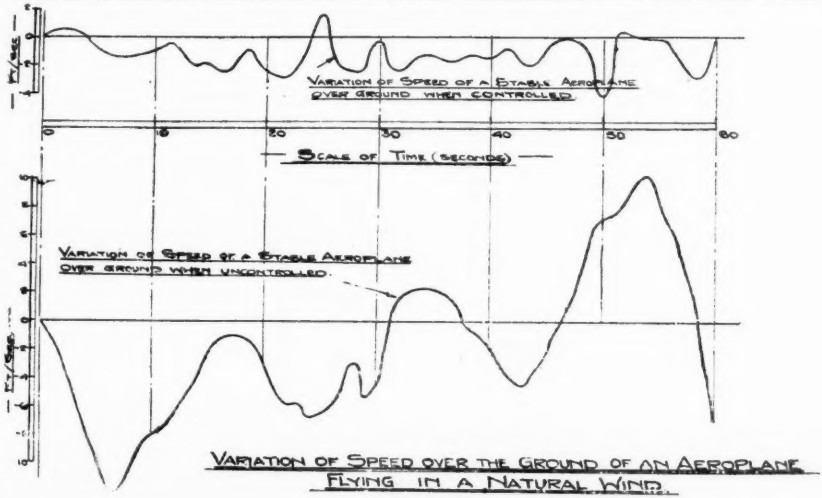


FIG. 20.

What is the "Factor of Safety."

It is often used, I think, in aeronautical writings in a manner not quite consistent with that in which it is employed by engineers. The normal stress coming on any part of the machine is taken as that which it has to bear in steady horizontal flight, produced, that is, by a loading equal to the weight of the machine, and if the breaking stress is N times this, then N is called the factor of safety. But a machine in its ordinary use may frequently have to carry a load much in excess of that which it bears in steady horizontal flight. It would be more consistent with engineering practice to estimate as best we can what is the maximum stress the machine in its daily use may have to bear and then take as the factor of safety the ratio of the breaking stress to this maximum stress. The factor of safety would thus take account of imperfections of workmanship or of material, not of the varying load. Thus, if we suppose that the maximum stress we ought to allow for is that due to a loading equal to N_1 times the weight of the machine—the normal loading in horizontal flight—and that the breaking stress is n times this, so that n is the true factor of safety, then the ratio of the breaking stress to that occurring during steady horizontal flight is nN_1 and—calling this N, so that $N = nN_1$—then N and not n is the factor of safety as ordinarily (but I think mistakenly) used in aeronautics. The procedure gives a fictitious value to the factor of safety.

The Determination of the Maximum Flying Stresses.

The value of N has been determined by calculation and in some cases by direct experiment for a number of machines, and appears to range from three to seven. Let us consider briefly whether these figures are sufficient and ask ourselves in the first place what are the maximum stresses a machine may be expected to experience in flight and what value we ought to give to N_1, the ratio of these maximum stresses to those arising from the weight of the machine. We may perhaps call N_1 the load factor or factor of loading. Let us consider first a machine flying at its maximum speed, and suppose the speed range is equal to the minimum speed, so that the minimum speed is half the maximum. When flying at minimum speed the angle of attack is of course a large one, and the lift coefficient in this attitude must be four times as great as when flying at its maximum speed. This might mean that the angle of attack was increased from some 3° to 12° or 14°, say through some 10°. If such a change could take place suddenly before the machine had time to change its attitude and adapt itself to its new conditions, since the lift coefficient is increased four times and the velocity is unchanged, then for the moment the stresses thrown on the machine are four times as great as those it experiences in steady horizontal flight at maximum speed. Now experiments on vertical air currents are not very numerous, but direct experiment shows that upward velocities of 10 to 15 feet per second have been observed, and these, if suddenly experienced, might be sufficient to alter the angle of attack by some 10°. Moreover, it appears possible that owing possibly to some obstacles or to the conformation of the ground the direction of the wind may alter—without change of speed—sufficiently in a distance comparable with a few lengths of an aeroplane to produce this change in the angle of attack. More careful investigation based on solutions of the equations of motion would give from three to four as the factor by which the loading may be increased, due to a sudden change in the angle at which the wind strikes the machine.

Another cause of serious sudden increase in loading is rapid flattening out after a dive.

Let us again take the case of a machine in which by an alteration of the angle of attack the lift coefficient may be increased in the ratio of 4 to 1. Calculation, and in one or two cases observation, have shown that it is possible for such a machine in a long steep dive to reach a speed approaching twice its maximum flying speed; the stresses are everywhere increased four times in

consequence, since they vary as the square of the speed; suppose now the elevators are put over quite suddenly as far as possible, so as to check the speed and bring the machine into the attitude relative to the air appropriate to minimum speed; the lift and hence the stresses are again increased fourfold. Thus, if this could be done instantaneously without gradually checking the speed, the stresses would be sixteen times those due to normal loading. Such sudden action is no doubt out of the question, but calculations, so far as it has been possible to carry them, show that stresses from eight to ten times those due to normal loading may be caused.

The Factor Required in Ordinary Flying.

Such high stresses of course do not occur in the ordinary conditions of flight if a machine is properly handled, and pilots should be careful never to allow the speed of their machines to exceed the maximum flying speed by more than some 15% or 20% and then only to put on the controls gently. I notice that Mr. Hucks, in a recent paper dealing with upside down flying, said that this was the instruction to which M. Bleriot attached the greatest importance. It is clear then that it is essential to make an effort to strengthen machines so that they may bear the stresses due to six times the normal loading at least and that even then occasions may occur in which this will be insufficient—occasions, that is, in which the real factor of safety will not exceed unity, even if it reaches that amount. An engineer would certainly think 2 too low a factor for his work and this implies that if N_1 the possible load factor* be taken as 6 the value of N would be 12.

The Need for a High Standard of Construction and of Piloting.

No doubt there are great difficulties in attempting to reach so high a value at present and indeed there are circumstances in which the advantages to be gained by saving weight cannot be neglected and which may always preclude its attainment. If it is not possible to secure that the machine cannot stand stresses more than six or eight times as great as those due to normal loading, all the more need is there for care in selecting the material and for skill in using it; all the more need for pilots to remember the conditions under which their machines are safe and to take care as far as possible to remain within the bounds set by those conditions. A prudent yachtsman does not sail his boat during the winter storms or risk its safety by an Atlantic passage; he knows its limitations. It may occasionally happen that a rapid dive and a sudden recovery are necessary, but these occasions are rare, and constructors can, I am confident, give our airmen machines which, if properly handled, have an ample margin of security.

I may be told I am asking for too high a standard. I am not capable of designing a machine and I have never flown, but I do not think the degree of safety I have specified is beyond our grasp, and I leave it to British manufacturers to reach it.

A man does not lose his reward either in this world or the next by setting a high ideal as the standard of his work and striving with his might to attain unto it. Wilbur Wright did that when he determined to fly.

The CHAIRMAN: I think we have all learned much from the very clear and able lecture to which we have listened.

The progress of aeronautics since I left England in 1907 has been extra-

* It should be noted that in calculating the stresses produced by a loading equal to say N_1 times the weight of the machine it is not sufficient to calculate them for the weight of the machine and multiply by N_1. The factor of loading N_1 must be settled before the calculations are begun and they must be made for a loading equal to N_1 times the weight. Moreover, allowance must be made for the motion of the centre of pressure as the attitude of the machine varies.

ordinarily rapid. A good many years ago I remember going on behalf of the War Office to see Mr. Maxim's flying machine, which ran along a railway and then took to the air. On that particular day it did not take to the air, though it ran along the railway quite satisfactorily.

Dr. Glazebrook has given us a clear idea of the wonderful technical evolution which has gone on since the Wrights made their first flights.

Many brains have co-operated in this evolution. The practical man, who is innocent of mathematics, but who has experience of flying, who knows what he wants and what are the weak points of his machine, has called upon the pure scientist to help him and has not called in vain.

I am glad to know that the National Physical Laboratory is doing such excellent work. If we are to take and keep the lead, as I believe we can, in this very important form of activity, it can only be by the harmonious co-operation of many minds contributing to the common stock of knowledge.

The experiments with models in air passages are very interesting, and I have no doubt that with the large seven feet channel even more valuable results will be obtained. The difficulty, no doubt, is to reason from the model in the confined air passage and stationary, to the aeroplane steered in the open and liable to changed conditions of air pressure as it rises or dives, or receives the wind at varying angles and varying speeds in the course in which it is directed.

There is also, even on calm days, the chance of striking suddenly a pocket of low pressure which at once alters the external forces upon the aeroplane.

I imagine that the conditions in flight cannot all be reproduced in air channels; but Dr. Glazebrook tells us that in the Paris Laboratory experiments have been made on full-sized aerofoils and repeated on models. The results show agreement as regards lift, which is very satisfactory.

The other method to which he refers seems hopeful. "The forces on an aerofoil are the resultant of the pressures due to the wind at all points of the surface," as he has told us. If these pressures can be measured at a sufficient number of points the pressure distribution can be plotted and the resultant force calculated.

It occurs to me, though I speak with great ignorance, that if these pressures could be measured in the case of an actual aeroplane supported, say, on the Eiffel Tower in a wind, useful figures might be obtained.

The problem of stability is most important. If an aeroplane can be produced which will automatically preserve its stability in all the conditions it is likely to encounter in the air an enormous advance will be made.

I gather from the lecture and from recent trials that this result has very nearly if not quite been attained, and that must be due largely to long and painstaking work and innumerable experiments which we must not forget. Mr. Bairstow and Mr. Nayler must be warmly congratulated on their scientific attack on these problems. Their mathematical analysis of the forces which operate upon an aeroplane is a fine piece of work which must help towards the complete solution which we may now hope to see.

Another problem with which Dr. Glazebrook has dealt is of vital moment: "What is a proper factor of safety to adopt?"

I rather gather that the high value of the Factor of Safety which theory indicates would be difficult to attain in practice because of the increase of weight involved.

I have had much experience when I was designing artillery carriages at Woolwich in difficulties of this kind, which, when one has utilised the very best material available and distributed it as well as we can, may still entail compromise.

Dr. Glazebrook holds out confident hopes that aeroplanes can be provided which "*if properly handled* will possess an ample margin of security."

It does seem to me that no unnecessary risks ought to be taken with a view to display. Aeroplanes ought to be flown only in the way that is necessary in order

to do what is expected of them—not to provide sensations for an expectant crowd, and I am sure that the Aëronautical Society will uphold the principle which Dr. Glazebrook lays down and will encourage pilots to remember the conditions in which their machines are safe and never to impose upon them unnecessary trials of strength.

I do think that we may congratulate ourselves on the progress we have made and are making. We were slow at the start, which is perhaps a national characteristic; but we have rapidly made up the time lost, and I believe it is just to say that as regards the science of aeroplane manufacture and the skill of our pilots we are now in the foremost rank. This is due to the co-operation of the Laboratory with the man of action to which I have referred.

I suppose that the philosopher will ask "What is the use of aeronautics to the cause of humanity?" It is too soon to answer that question. We do not yet know the extent of the services which the aeroplane will render. Meanwhile, for naval and military purposes we see a large sphere of usefulness, and already we recognise that the aeroplane is an essential part of the equipment of a navy and an army.

But there is something more than this! In an age when ease, comfort, and security, to be obtained with the least amount of risk and exertion, are the demands of large classes, it is surely something that a new class of courage and of endurance has been brought into play.

Of Wilbur Wright and those who followed him it may be said with more force than of the first navigators *robur et æs triplex circum pectus erat*.

Death has laid a heavy toll on the pioneers of aeronautics. Always new men have come forward to take the place of those who fell, and who deserve to be honoured as much as those who gave their lives in battle.

We may now hope that the risks will be reduced to a minimum and this will be due to the high courage of the pioneers whose efforts we commemorate to-night.

To the memory of Wilbur Wright no better tribute could be paid than the lecture to which we have listened, and I have the greatest pleasure in proposing a hearty vote of thanks to Dr. Glazebrook and to the Staff of the National Physical Laboratory, of which he is the distinguished head. (Loud applause.)

Dr. Glazebrook having suitably acknowledged the vote of thanks,

Major-General R. M. Ruck, C.B., in proposing a vote of thanks to the Chairman, said that it was not the first occasion on which Lord Sydenham had assisted the Society. They would remember that a few months back he had taken the chair at Colonel Sykes' lecture on "Military Flying." It was decidedly remarkable that for two lectures dealing with such essentially different subjects Lord Sydenham should have been so pre-eminently the man to preside. This was due to his unique knowledge of military requirements accompanied by a large experience of scientific methods and power of grasping the essentials of any problem with which he was brought into contact. The Society was deeply grateful for the interest he had taken in its proceedings, and he was sure they all hoped that to-night would be by no means the last time that he would preside at one of its gatherings.

Before he sat down he would like to add his congratulations to Dr. Glazebrook for his admirable lecture, so admirably clear that even he (General Ruck) could appreciate its immense value to those concerned with the theory and practice of aeronautics. The previous night the Society had had much praise from many distinguished men for the scientific work of its members. Personally, he felt a complete impostor amongst such associates, but would yield to none in his admiration for the work done by members of this Society. It was marked with a thoroughness and enthusiasm such as would, he was sure, have commended itself to Wilbur Wright had he been still with them.

On behalf of all present he desired to tender their warmest thanks to Lord Sydenham for presiding and for his most valuable remarks and the kind words he had spoken concerning the Society. (Loud and prolonged applause.)

The meeting terminated at 9.55 p.m.

THE HORIZONTAL WIND TUNNEL AT EAST LONDON COLLEGE.

BY A. P. THURSTON, D.SC., A.F.AË.S., AND N. TONNSTEIN, B.SC.

The horizontal wind tunnel at East London College consists of a wooden tube, 2 feet square in section by 11ft. 6in. long, through which air may be drawn at velocities up to 50 m.p.h. by means of a 6½ h.p. motor.

It was designed and constructed by Messrs. Cedric Lee and G. Tilghman Richards from sketches supplied by one of the authors and was modified later by the addition of a wind disperser as the result of information kindly supplied by Dr. Stanton, of the National Physical Laboratory. This disperser was added principally with the object of reducing the draught in the room and of preventing pulsations of the air current.

The tunnel is provided with a bell mouth to ease the air into it, and at the other end it is connected to the disperser by a metal duct, 3ft. 6in. long, expanding from 2ft. square to 3ft. 6in. diameter. The propeller is mounted in the enlarged end, which is cylindrical for some 7 inches, so as to draw the air through the tunnel.

The disperser is 9ft. long by 4ft. square and is closed at the far end. The top, bottom and sides are formed by a large number of wooden strips spaced according to the following scheme, reading from the propeller end:—

16	strips	⅞in.	wide	and ⅞in.	gap.
21	,,	⅜in.	,,	½in.	,,
40	,,	⅝in.	,,	0.2in.	,,

The disperser is not as long as would be desirable owing to lack of space.

The specimen is mounted in the tunnel 6ft. 4in. from the intake at the end of a beam, Fig. 5, about 4ft. 6in. long, passing at right angles through the side of the tunnel. This beam is mounted in gimbals near its centre which provide for a vertical and horizontal movement of the specimen. The force producing the first of these movements is measured directly, by means of a jockey weight, on the main beam, which may therefore be called the lift beam of the balance. That producing the second is transmitted to a second measuring beam, Fig. 6, placed at right angles to the lift beam, by means of a "knife" wheel engaging a hardened steel plate carried by a depending arm. Simultaneous measurements of lift and drag may be taken by this arrangement. The balance is mounted in an angular case projecting from the side of the tunnel, as shown in the figures.

A separate beam, placed 2 feet further back, is used for determining the centre of pressure, the line of resultant force, and the rolling, pitching and yawing moments of a model.

The maximum variation of velocity over an area 1ft. square in the centre of the tunnel is 1% on either side of a mean velocity of 34 m.p.h.

The wind velocity is ascertained by measuring the suction at a hole in a brass plate, flush with the bottom of the tunnel and immediately under the specimen, a method kindly communicated to one of the authors by Dr. Stanton. The suction is measured by a Xylol micro-manometer, an arrangement which allows reasonably accurate "sight" readings of velocity to be taken rapidly. The combination has

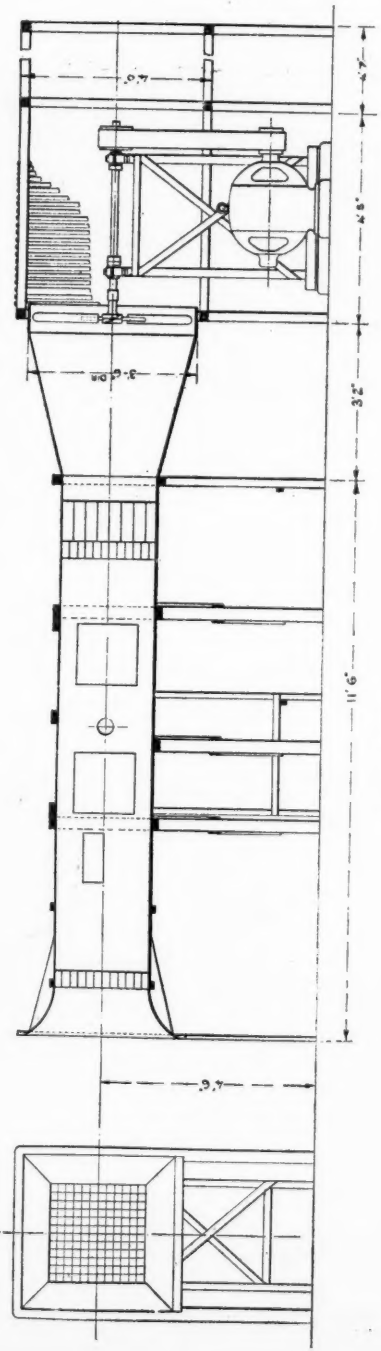


FIG. 1. END VIEW (INTAKE).

FIG. 2. SIDE VIEW (SECTION).

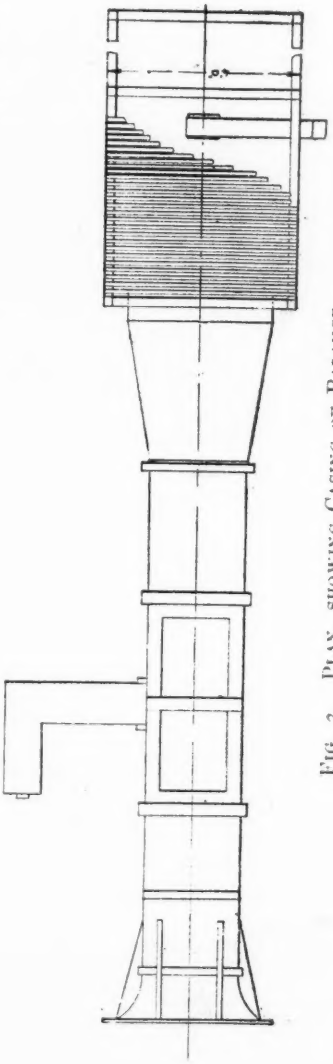


FIG. 3. PLAN, SHOWING CASING OF BALANCE.

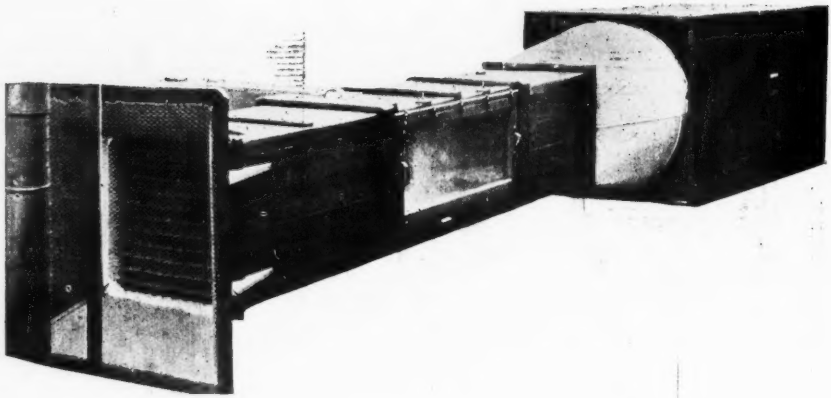


FIG. 4. GENERAL VIEW OF WIND TUNNEL.

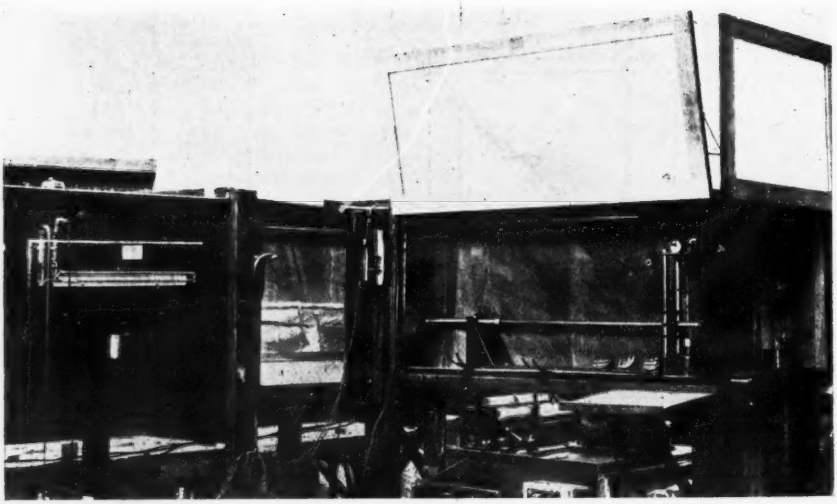


FIG. 5.

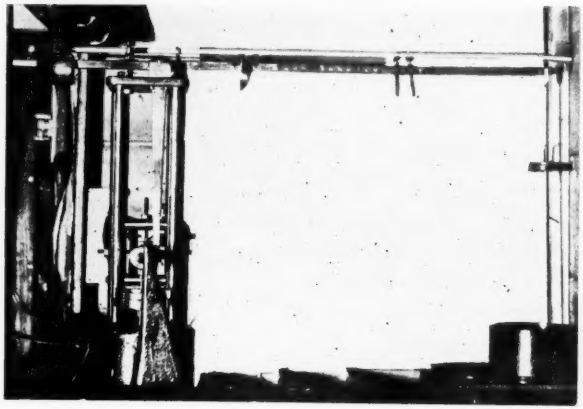


FIG. 6.

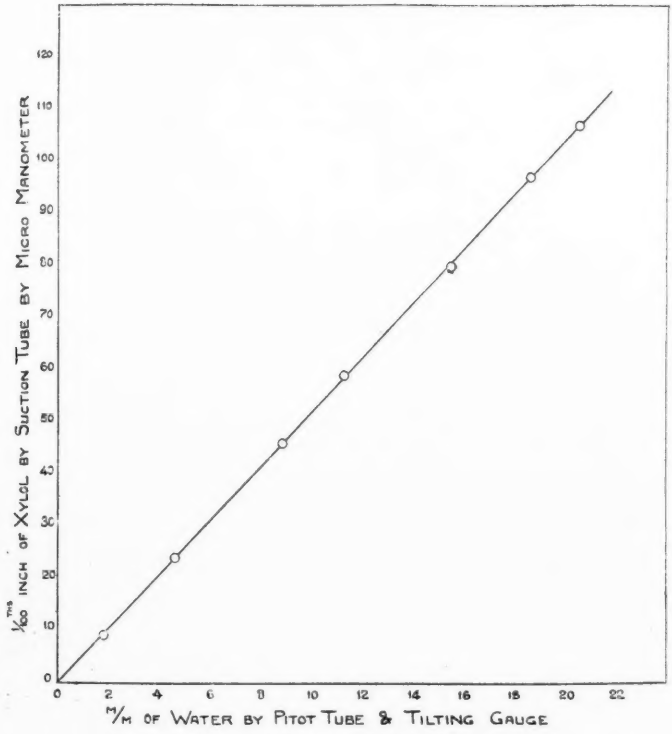


FIG. 7. CALIBRATION OF VELOCITY MEASURER.

been calibrated by a Pitot tube connected with a sensitive tilting water gauge, and it will be seen from Fig. 7 that the relation is a straight line law over the whole range of velocity in the tunnel.

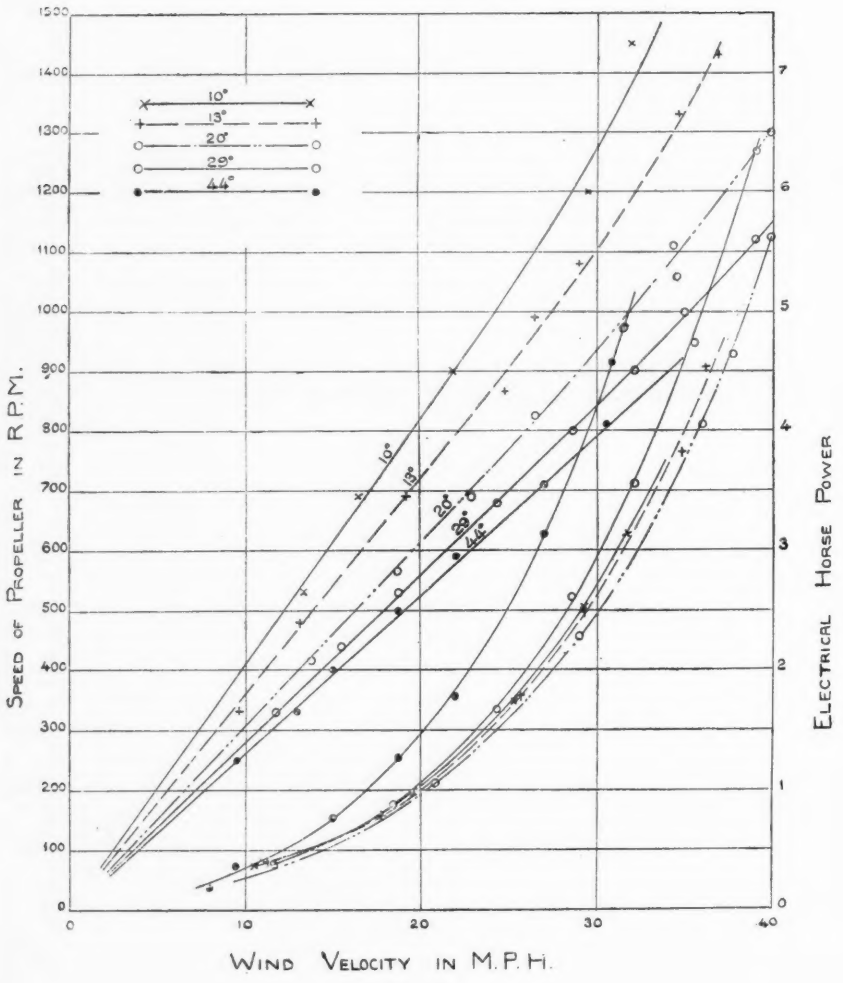


FIG. 8.

Consequently the relation can be expressed by the following:—

Pitot tube reading = .869 suction reading.

The suction tube obviously offers a more sensitive means of measuring velocity since it gives a larger difference of pressure than the Pitot tube.

The propeller is 3ft. 5in. diameter and is fitted with four metal blades, 17in. long by 4in. wide and cambered 0.3 inches the whole length along the centre lines. The blades have been made adjustable and the effect of varying the angle has been investigated. For each test the blades were set at a certain angle and the speed of the propeller was gradually increased. Readings of wind velocity, fan speed, and electrical h.p. consumed by the motor were taken simultaneously and are shown plotted in Fig. 8. The last quantity was measured

by means of a delicate voltmeter and ammeter placed across the terminals of the motor and therefore includes frictional and electrical losses.

Tests might have been run to eliminate these, but this paper is intended primarily to be of service to those designing wind tunnels.

In Fig. 9 the wind speed has been plotted against the inclination of the blades for various powers.

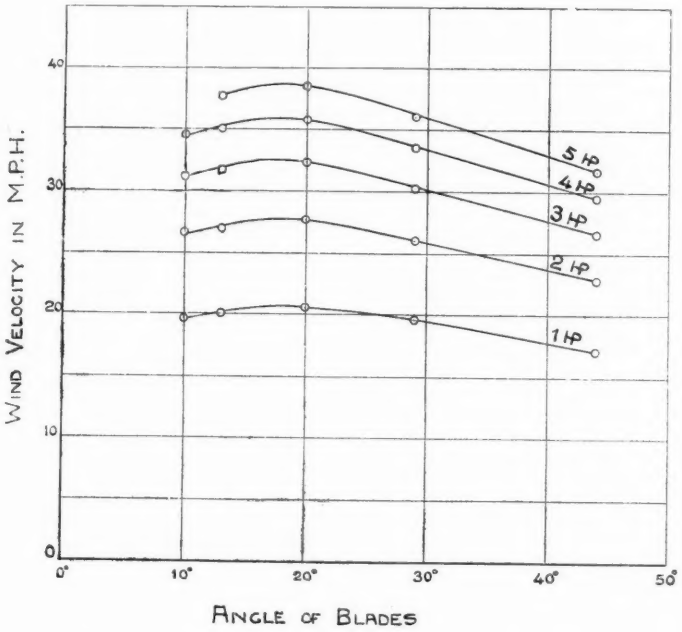


Fig. 9. Relation of Blade Angle to Wind Speed.

The results of the experiments may be summed up as follows.

The method of measuring the wind velocity in the tunnel is accurate, but requires calibration.

A fairly uniform wind current up to 50 m.p.h. can be obtained with the h.p. available using a wind disperser of the N.P.L. pattern.

The wind speed is proportional to the revolutions of the fan up to velocities of about 30 to 40 m.p.h., depending on the angle of the blades.

With the type of propeller used the best efficiency is obtained with the blade angle about 18°. The inclination can be varied over a considerable range, say 15° to 20°, to suit a desirable fan speed without serious loss of efficiency.

In conclusion the authors desire to thank Mr. Cedric Lee for a generous grant towards the construction of the tunnel.

BRITISH FATALITIES.

Date—1914.	Name.	Nationality.	Machine.	Place.
April 26.	Philippe Marty.	French.	Morane-Saulnier(1).	Hendon.
May 12.	Capt. E. D. Anderson.	English.	Sopwith (2).	Farnborough.
May 12.	Air Mech. Carter (P).	English.	Sopwith (2).	Farnborough.
May 15.	Lieut. J. Empson. Air Mech. Cudmore (P).	English.	B.E.	nr. Northallerton.
May 23.	Gustav Hamel.	English.	Morane (1).	Lost in English Channel.
June 4.	Lieut. T. S. Cresswell. Comm. A. Rice (P).	English.	Wight seaplane.	Off Calshot.
July 20.	Lieut. R. C. Hordern.	English.	Henry Farman (2)	Fort Grange, Gosport.

(1) Monoplane. (2) Biplane. (P) Passenger.

NEW BOOKS AND PUBLICATIONS.

L'Essor et l'Atterrissage. Maurice Percheron. Paris: Dunod et Pinat. 1914. pp. XII. + 143. Figs. Price 4 frs. 50.

Hydromechanic Experiments with Flying Boat Hulls. H. C. Richardson. Washington: Smithsonian Institution. 1914. pp. 9 Illus.

Die Arbeiten des Konigl. Preuszischen Aeronautischen Observatoriums bei Lindenberg im Jahre 1913. Dr. R. Assmann. Braunschweig: F. Vieweg and Sohn. 1914. pp. XLVIII. + 544. Price M. 25. Illus.

Advisory Committee for Aeronautics. Reports and Memoranda 96. Report by the Committee as to the Strength of Construction Desirable in Aeroplanes. January, 1914. pp. 5.

Technical Report of the Advisory Committee for Aeronautics, 1912-13 (with Appendices). London: H.M. Stationery Office. 1914. pp. 416. Figs. Price 10s.

Chronology of Aviation. Hudson Maxim and W. J. Hammer. New York. Reprinted from the World's Almanac for 1911. pp. 23.

La Traversée aerienne de l'Atlantique. MM. A. de la Hault and A. Bracke. "La Conquête de l'Air." 1914. pp. 30. Illus. Price 45 cents.

The Viscosity of Liquids. A. E. Dunstan and F. B. Thole. London: Longmans, Green. 1914. pp. 89. Figs. Price 3s.

International Meteorological Committee. Report of the Tenth Meeting, Rome, 1913. London: H.M. Stationery Office. 1914. pp. 98. Price 2s.

The Year Book of the Royal Society, 1913. London: Royal Society. 1913. pp. 258. Price 5s.

Osterreichischer Aero Club Jahrbuch, 1914. Vienna: Osterreichischer Aero Club. 1914. pp. 332.

REVIEWS.

Animal Flight: A Record of Observations. By E. H. Hankin, M.A. Sc.D. (London: Iliffe & Sons, Ltd. 1913. pp. 405. Illus. 12s. 6d.)

This is an interesting book, as it contains a great deal of comparatively little-known information on certain kinds of flight. It is somewhat difficult to study, as the details of the different points discussed are scattered about the

various chapters; but a great deal of useful information is given, and the book is undoubtedly well worth perusal by those interested in "Animal Flight."

The main subjects discussed: These are "Soaring Flight"; the general action of the wings during flight; use of the tail; notes on flapping flight; the flight of bats; flying fish; dragon flies, etc., etc.

The author's observations: These were made chiefly at Agra and Naini Tal; velocities and flights were measured instrumentally, and a very ingenious method of recording the track of the bird's course on a looking-glass surface was made use of.

As regards the birds, the most important observations were those on soaring birds—viz., the cheel, the scavenger, common and black vultures, and the adjutant. The loading of all these birds was very light—viz., from 1.54 lbs. per square foot for the adjutant to .55 lbs. per square foot for the cheel.

"*Soaring Flight*": This is classified under two main heads—viz., "True Soaring" or "gliding flight, in which birds or other soaring animals appear in some unknown way to get energy from the air," and "False Soaring" or "gliding in which the bird takes advantage of ascending currents known to exist."

As regards "True Soaring," this, again, is of two kinds—viz., "Sun Soaring," which occurs in the presence of sunshine, and "Wind Soaring," which may occur in the absence of sunshine, but only in the presence of wind; this latter type of soaring not being, in the opinion of the author, dependent upon the velocity of the wind. It is particularly pointed out that in "True Soaring" the birds do not remain in a restricted area, and in many cases appear to glide longitudinally for indefinite distances. Attention is drawn to the fact that this type of soaring only takes place for a few hours after sunrise till an hour or two before sunset, and that a good deal of bright sunshine is necessary for the best flights. The birds in this case appear to avoid ascending currents.

"False Soaring" depends upon the velocity and direction of the wind; the area in which it is practised is always restricted, and the height to which the birds rise is limited.

The paths traced out in soaring flight may be straight lines or curves, and diagrams showing the character of such paths, both in calm and disturbed air, are given. Attention is drawn to the great regularity of all these movements: the curved paths seem to be almost always of the same dimensions, and there is very little variation in the mean velocities. As regards the heights attained, the birds with the heaviest loading are usually those seen at the greatest heights. Tables showing their heights are given on pp. 19 and 20.

Turning next to the condition of the air when "True Soaring" takes place, the author is not at present able to say much. He states: "I have employed the words 'soarable' and 'soarability' as applied to the air. These words are necessary for purposes of description, and imply nothing as to the reason why sometimes the air can and sometimes cannot furnish the energy necessary for soaring flight. To speculate on the reason for this difference is at present premature."

Wing Movements: As is well known, the wing of a flying animal acts as a sort of flexible paddle, alterations in the form of which are due either to the natural elasticity of the parts composing it or to voluntary muscular movements on the part of the animal itself. The alterations in the position of the wing relatively to the body are due to muscular exertion, and, though sometimes overlooked, are a very important factor in flight.

As shown by the author, the whole of these alterations in wing area and wing movement vary with the particular kind of flight practised, and a very full account of these movements, etc., is given. Steering and directive movements

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generally are considered in Chapters IV. and VI., methods of descent in Chapters VII. and X., "arching" or concavity of the wing in the lateral direction in Chapter VIII., and certain factors affecting the angle of incidence of the wing in Chapter XI. The results of the observations confirm generally those of the older observers, such as D'Esterno, Mouillard, Lilienthal, and others, while some additional observations regarding "canting" (see App. I.) are given. This part of the book is very fully developed, and the descriptions given of the peculiar wing movements made for particular flight manœuvres are of great interest.

Use of the Tail: As regards the tail, the author seems to agree with Lilienthal, whose opinion he quotes. The tail does not act like the vertical rudder of an aeroplane, but appears to be chiefly used as a brake. In certain small birds it is always expanded, and forms one of the "three supports" so often quoted by French writers.

Flapping Flight: Some details regarding this style of flight are given in Chapter IX. There are good descriptions of "poising," "settling down," etc., and a discussion on the general position of the wings in slow, medium, and fast flight. A table of the number of beats per second, and diagrams of the range of beat, in certain kinds of flight are given. This chapter is of great importance, and it is to be hoped that the author will develop the subject further, especially in the direction of ascertaining the connection between the number of beats and the forward velocity, and the variations of the to and fro velocity of the wings relatively to the body.

Miscellaneous Types of Flight: Special chapters are devoted to the flight of seagulls, bats, flying fish, and dragon-flies; they will all repay perusal.

Colour Phenomena: Some curious details as to apparent changes in the colour of the wings in soaring flight are given in Chapter XVIII. The author considers that this subject may possibly be worth studying, when investigating the mechanism by which the sun energy is stored in the air.

Relative Efficiency of Wing Forms: This is considered in Chapter XIX. There are good diagrams showing the sections of different kinds of wing.

Remarks: The main interest in the book for most readers will be the question of "Soaring Flight." The author does not give any solution of the problem, but he gives a number of details to show that the flying animals appear in some unknown way to get energy from the air, true soaring flight being, according to his views, quite independent of the wind's velocity and direction. This may possibly be the case, but it is doubtful whether he has given enough consideration to the effect of the sun in causing vertically rising currents in places like Agra, where the daily rises of temperature seem to coincide almost exactly with the hours at which the birds start and practise soaring. At Naini Tal, too, there must be strong upward currents, and it seems extremely doubtful whether there is calm air (viz., air absolutely at rest) in either of these places during the hours soaring occurs. With such lightly loaded birds, upward currents having a vertical velocity of from two to four feet per second are ample for support, and it is only by very delicate instrumental observations that such currents could be detected. (See the discussion on "Soaring Flight" at the Aeronautical Society's meeting, *Aeronautical Journal*, April, 1912, pp. 79-80.)

There is an excellent Appendix, which should be carefully studied before commencing to read the book. The hand sketches and illustrations in the text are particularly good.

J. D. FULLERTON, Colonel, R.E. (ret.).

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